MEDICAL REPOSITORY.

VOL. IV.—No. IV.

ARTICLE I.

On the YELLOW FEVER *at Norfolk (Virginia), in the Summer and Autumn of* 1800: *Communicated by Doctors* SELDEN *and* WHITEHEAD *to Dr.* MILLER.

TO determine the origin of the malignant fever, which has, of late, ſo frequently viſited the cities of the United States, whether it be foreign or domeſtic, is certainly a matter of the greateſt importance. A diligent and careful examination of circumſtances, a fair and candid ſtatement of facts, free from the heat of controverſy, and the acrimony of conteſted opinions, are undoubtedly the beſt means of ſhedding light upon this dark and difficult ſubject. Impreſſed with this belief, we will ſtate, as briefly and clearly as we can, the ſituation of Norfolk, the local circumſtances, both permanent and accidental, previous to the appearance of the diſeaſe, and during its progreſs, which have been known, in other places, to affect the health of the inhabitants; leaving it to thoſe who have accurately examined the operations of nature, in ſuch caſes, to form an eſtimate of the influence of each cauſe ſeparately, and jointly with the reſt, and determine whether we ſhall, with moſt juſtice, conſider this fatal malady the offſpring of our own climate, or regard it as a direful diſtemper, introduced by commerce from a foreign country.

The ground upon which the principal part of the town of Norfolk ſtands, forms a kind of peninſula of an oblong figure. The main ſtreet, ranging with the courſe of Elizabeth river, runs along the ridge of this peninſula, forming its longeſt diameter. It is not paved, nor are any of the other ſtreets or alleys, one ſmall lane only excepted; and the ſoil being chiefly a red clay, mixed with ſand, renders them, in rainy weather,

half an inch diſtant from the boundary of the red colour, and the heat at one inch was equal to that of the middle of the red colour itſelf. The boundaries of the *calorific ſpectrum* lie between the extreme of violet and an undetermined point, at leaſt 1½ inch beyond the red colour. From the above very important facts it follows, that there are rays coming from the ſun which are leſs refrangible than any that affect the ſight; that theſe *inviſible* rays of the ſun probably far exceed the viſible ones in number; and if we call *light* thoſe rays which illuminate objects, and *radiant heat* thoſe which heat bodies, it may be queſtioned whether light is eſſentially different from heat. [*Lond. Month. Mag.*

CURE OF SYPHILIS BY OXYGEN.

The late Mr. Girtanner writes, in a letter to Van Mons, that he has made a number of experiments in curing venereal diſeaſes by oxygen, which diſcovery he claims to himſelf. The reſults of theſe are the following: When the diſeaſe is not inveterate, and only requires the firſt degree of oxydation, he uſes the citric acid; for the ſecond degree, the diluted oxalic acid; for the third degree, and, in general, againſt the moſt inveterate diſeaſes, he applies the oxyd of arſenic. There is no remedy more efficacious againſt venereal diſeaſes, diſeaſes of the liver, obſtructions of the belly, dropſies, &c. than this; but the lungs muſt not be affected, as the patient cannot then ſupport it, but will expire in a ſhort time. He mixes four or five drops of a ſaturated ſolution of the white oxyd of arſenic in nitric acid, with two pints of water, to be taken in two days. This remedy has done wonders with him. There is alſo nothing more powerful againſt agues than this. If the patient begins to cough, the remedy muſt be laid aſide, a dry cough being a ſign of the body becoming over oxygenated. Should the cough continue, it can be moſtly removed by the liver of ſulphur. [*Hufeland's Journal, vol. ix. No.* 102.

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extremely muddy. The market, where butchers' meat, poultry, and vegetables of all kinds, are ſold, ſtands on the main ſtreet, near the middle of the town; around which are ranged every morning carts from the country, loaded with proviſions of every kind, both vegetable and animal. It is generally after eleven o'clock (A. M.) in ſummer, before all proviſions are removed from the market place, ſo that, from what of animal and vegetable ſubſtances is left on the ground, by thoſe attending market, and from the ſweepings of the ſtores, houſes, kitchens, &c. which are caſually thrown into the ſtreets or gutters, eſpecially in the more populous parts of the town, we may juſtly conſider their ſurface as a compoſt of animal and vegetable matter, kneaded together by the feet of paſſengers, horſes, drays, &c. which, when acted on by moiſture, and the ſcorching rays of a burning ſun, muſt exhale abundance of gaſes extremely deleterious to the health of the inhabitants. This might be ſuppoſed to be true in a particular manner, this ſummer, from the long continuance of an extreme degree of heat and moiſture; which was, in ſome meaſure, confirmed by the obſervation of ſtrangers, who, on their firſt arrival in town, complained greatly of the diſagreeable ſmell of our ſtreets and gutters, though it was not ſo perceptible to the inhabitants of the place. But a more fertile ſource of noxious exhalations remains to be pointed out. The line which marks out the boundary of the borough of Norfolk, on the ſide next the river, comprehended originally more water than land, on that ſide of the main ſtreet. In ſome places the wharves are advanced upwards of an hundred yards into the river, as far as this boundary line, in others they are not yet carried ſo far, while, in ſome parts, no attempt has yet been made to diſturb the original poſſeſſion of the river; but that part of the town where the malignant fever chiefly prevailed, ſtands entirely on made land, reclaimed from the river, by ſinking pens of large logs, and filling them up chiefly with green pine-ſaplings, which are ſlightly covered over with earth or gravel. In ſome places large openings are left for the formation of docks; in others, wharves are formed next the channel of the river, while the more interior parts are ſtill covered with water, and, in many others, the lots remain in their original ſtate; ſo that from theſe circumſtances, and the looſe texture of the whole work, the water of the river penetrates, every tide, through the whole extent of this wooden fabric, which is thus alternately expoſed to the action of the water and air, aſſiſted by the powerful rays of an almoſt vertical ſun. The private

intereſt of the moment, without regard to futurity, or interference of the police, ſeems alone to have directed the arrangement of the houſes on this property reclaimed from the river. The means of ventilation and cleanlineſs have been neglected; proper ſpaces for ſtreets have not been left; and what has been marked out for that purpoſe, is, in ſeveral places, not yet filled up, though the private property around is completely occupied with buildings. In the only tolerable public ſtreet in this part of the town, there are two places, in a very public ſituation, about twenty yards each in length, and eight or nine in breadth, which have never yet been filled up with other materials than what accident has carried thither, and the filth of the neighbourhood, for which it ſerves as the common and only receptacle. In theſe ponds, or rather ſinks, of putrefaction, though ſixty yards or more from the river, the tide ebbs and flows through the open texture of the adjacent reclaimed ground; yet, at low water, they are never entirely dry, but form an agreeable retreat to the neighbouring hogs to wallow in, as is cuſtomary with theſe animal in the ſultry months of the year. The effluvia which were exhaled from theſe and ſimilar places, during the warm months of ſummer and autumn, were indeed highly offenſive, and will appear to many medical men almoſt ſufficient of themſelves to account for the generation of peſtilential diſeaſes, wherever they may exiſt. And, in fact, the malignant fever did firſt make its appearance about twenty yards from theſe offenſive pools, in a wooden houſe, reſting upon logs, the ground under it not being raiſed to a level with the adjacent land, and which was always covered partly with water, and completely overflowed twice in twenty-four hours by the tide. Beſides theſe, and many other ſources of peſtilential vapours, which abounded in this quarter of the town, another, at this time, was accidentally added. Three veſſels arrived from the Weſt Indies ſucceſſively, on the 21ſt, 22d, and 23d of July, loaded partly with fruit. Theſe cargoes were landed on Commerce-ſtreet wharf, about ſixty yards from the above deſcribed ponds, and the greateſt part of the oranges and limes being ſpoiled, the caſks were opened, and picked on the quay, and the rotten fruit, together with the matter in which they were packed, were left on the ſpot a great many days. Theſe formed a large maſs of vegetable matter, in a ſtate of putrefaction, the offenſive ſmell ariſing from which was ſo great, that gentlemen have declared they were unable to paſs that way without ſtopping their noſtrils; and in one perſon, of undoubted veracity, deliquium animi, as he affirmed, was

well nigh induced from the ſame cauſe: yet this nuiſance was not removed till after a number of perſons ſeized with a malignant fever in the neighbourhood excited alarm, and attracted that way the attention of the public.

Two young gentlemen employed in a vendue-office at the ſpot where the fruit was landed, and by whom it had been picked, and repacked for public ſale, and near the window of whoſe chamber the above mentioned putrid vegetable maſs had been for ſome time lying, were the firſt attacked on this ſtreet, on the 26th of July, and both dying, after every effort had been made by their phyſicians to ſave them, with the uſual ſymptoms of the Yellow Fever, in its moſt malignant form, occaſioned no ſmall apprehenſions. Several, indeed, had died, after a ſhort illneſs, ſome days before, but theſe being moſtly ſailors, in obſcure boarding-houſes, and not regularly attended, we believe, by any phyſician, had drawn but little attention.

With regard to the diſeaſes which prevailed anterior to the appearance of the malignant fever this year in Norfolk, it is worthy of notice that complaints of an high inflammatory character very generally obtained, during the ſpring and firſt of the ſummer months, attended with ſymptoms of greater ſeverity than were uſually met with in other years.

The ſmall-pox, which had not diſappeared altogether in the winter, became, in a manner, epidemic in the beginning of ſpring. Many were attacked with this diſeaſe in the natural form, and died, who thought themſelves ſecure from infection, believing to have had the diſorder in their youth, as they had frequently acted as nurſes, with perfect ſafety, to thoſe labouring under the complaint. From inoculation alſo this diſtemper was uncommonly ſevere: large ſores formed frequently at the place of inoculation on the arm, and tumors and abceſſes in other parts of the body, which often proved extremely obſtinate in the cure. In the month of May ſeveral caſes of cynanche maligna occurred, ſome of which had a fatal termination. The whooping-cough made its appearance early in the ſpring; in April and May it became very general, ſevere, and often proved fatal; nor did its violence abate till towards the end of July. The cholera infantum began very early in June, and carried off vaſt numbers of children. Slight caſes of dyſentery, and ſevere caſes of cholera morbus were common in June and July.

The influence of climate in the generation of malignant diſeaſes is acknowledged by medical men of every age; and the preſent year, in this place, it appeared to be extremely un-

friendly to health. The ſpring was uncommonly late, but the hot weather ſet in very early. On the 8th of April the quickſilver had riſen as high as 90 degrees of Fahrenheit; about the 20th of June it became ſteadily hot, and continued till after the middle of Auguſt with little variation. Southerly winds prevailed, which were always light, and often interrupted with calms; nor did we once experience, during that period, the north-weſt wind, which uſually ſucceeds rains, and tornadoes at this ſeaſon; which proves ſo refreſhing and invigorating, and which ſuſpends, for ſome time, the too powerful influence of the ſun in a ſultry climate. Rains had been very frequent in June and July; but inſtead of cooling the atmoſphere, it was obſerved that they were invariably followed by more intenſe heat. For more than two months after the 25th of June, the inhabitants of Norfolk lived in an atmoſphere heated above the 85th degree of Fahrenheit, ſometimes to the 94th and 95th, but very frequently upwards of 90 degrees. Such a long tract of intenſely hot weather is not, we believe, within the remembrance of any perſon now living in this place; and its effects on the health of the inhabitants, muſt, of neceſſity, have been very conſiderable. It has been already obſerved, that, previous to the 20th of July, ſeveral perſons had died, after a ſhort illneſs, in a houſe adjoining the ponds of water above deſcribed, without exciting any particular notice; but in a few days they were recollected with ſenſations of dread, when a diſeaſe of malignant aſpect began to ſpread rapidly in the ſame part of the town. On the 22d of the ſame month, while yet no ſuſpicion exiſted of an epidemic complaint, we were called to ſee Thomas Bailey, a ſailor, then in the third day of his illneſs—His eyes were much inflamed and watery, his tongue was moiſt and furred, and his countenance expreſſive of great anxiety and diſtreſs; he was extremely reſtleſs, and complained of intolerable pain over his eye-brows, great oppreſſion about the præcordia, with a ſenſation of burning in the region of the ſtomach, which made him feel, he ſaid, "as if his bowels were all on fire." The heat of the body was much increaſed above the natural ſtandard, his face was fluſhed, and his pulſe was more remarkable for frequency than ſtrength. He had taken ſome evacuant medicines before we ſaw him. He was bled with manifeſt relief of his head-ache; a purge of calomel and jalap was exhibited, and a bliſter was applied to the region of the ſtomach. By theſe means the febrile ſymptoms were conſiderably moderated; but the bliſter was not ſuffered to remain till veſication was accompliſhed, and

the ſenſation of burning in the region of the ſtomach increaſing, vomiting became inceſſant, and of a dark colour; the ſtrength ſunk rapidly, while the ſkin aſſumed a yellow hue, and the diſeaſe haſtened to a fatal termination, with ſymptoms unuſual in the febrile diſeaſes of ordinary ſeaſons. We have been induced to notice this caſe more particularly in conſequence of a belief with many that the malignant fever was introduced by the veſſels which arrived from the Weſt-Indies on the 21ſt, 22d and 23d of July. Although the extreme degree and long duration of the hot weather, with ſome other unfavourable circumſtances, which have been mentioned already, had not excited, for ſome time paſt, among the medical gentlemen, apprehenſions of a fatal epidemic; it was impoſſible to help obſerving, during the progreſs of Bailey's caſe, the ſtrict reſemblance it bore to the Bilious Yellow Fever, as deſcribed by phyſicians in different parts of the United States, where it has made its appearance. Our apprehenſions were but too ſoon realized. In two or three days after the termination of the above mentioned caſe, almoſt every phyſician in town had patients labouring under the ſame diſeaſe. Many of thoſe who were firſt attacked ſunk under the diſeaſe; and ſo great was the alarm in that quarter where it firſt appeared, that moſt of the mercantile houſes in Commerce-ſtreet, were, in leſs than a week, entirely ſhut up; and thoſe, who, from circumſtances, thought themſelves particularly liable to the complaint, left the place as ſoon as poſſible.

Europeans and natives of the northern States, who had not been accuſtomed to warm climates, were moſt expoſed to the attack of the diſeaſe in its ſevereſt form; thoſe from the ſame countries who had reſided here ſome time, and ſtrangers from this and the neighbouring States, were not exempt, but the diſeaſe put on a milder form; while thoſe who were born in Norfolk, and were old reſidents of the place, never enjoyed a greater portion of health in any former ſeaſon; none of them died, or were even affected with the prevailing epidemic. This entire exemption of the permanent inhabitants of Norfolk, different from what was experienced in more northern parts of America, as Philadelphia, New-York, and Baltimore, may probably be accounted for on the ſuppoſition that our ſituation and climate here approach nearer to the circumſtances of the Weſt-India iſlands, where ſtrangers in general are the only perſons attacked with the Yellow Fever.

Habitual intemperance in the uſe of ſpirituous liquors was the moſt powerful prediſpoſing cauſe; and in a vaſt number

of inſtances, the diſeaſe was excited by a fit of intoxication, in which caſes the greateſt mortality occurred. But the fatal ravages of the fever were greatly extended by the very bad nurſing and attendance given to the ſick. In moſt of the ſmall boarding-houſes, where ſeafaring people are received, the rooms are confined, and the accommodations wretched. The care of the ſick was almoſt wholly entruſted to negroes, who, not being prompted by humanity, attachment, or a ſenſe of duty, executed their truſt in a very negligent manner; nay, in more inſtances than one, the people of the houſe deſerted their habitation entirely, and left the ſick to their fate. No wonder, in ſuch circumſtances as theſe, that the efforts of the phyſician were often unavailing, when deſtitute of that which is probably more neceſſary in this than any other diſeaſe, the co-operation of a careful and a judicious nurſe.

Tobacco ſhips were remarkably unhealthy, although communication with the ſhore was, as much as poſſible, guarded againſt. Of ſeveral that were loading here, during the months of Auguſt and September, a great part of their crews ſickened and died. Nor was the opinion entertained by many, that negroes are not ſuſceptible of the diſeaſe, found correct; many of them were attacked, and ſeveral fell victims to the diſeaſe. For ſeveral days in the laſt of July, and early part of Auguſt, the weather was cooler than it had been for ſome time before; there was then a temporary ſuſpenſion of the diſeaſe; but we are at a loſs to ſay whether this was owing to the change of weather, or to the inhabitants avoiding that part of the town which had been infected in the beginning; for the fever for ſeveral weeks after its commencement was quite local. The weather became ſteadily hot again about the 6th of Auguſt, and continued ſo throughout that month; the diſeaſe returned with the hot weather, and continued with unabated violence to the end of the month; by the middle of Auguſt the Yellow Fever was the only diſeaſe we met with in our practice; on the 30th there was a conſiderable fall of rain, which was followed by three or four days of cool weather, with a briſk northerly wind. On the 4th of September the thermometer ſtood at 60 degrees in the warmeſt part of the day. Attacks of the fever were leſs frequent at this period, but there was little alteration as to the form or ſeverity of the diſeaſe. The number of caſes was diminiſhed evidently from one cauſe, the leſſened number of ſubjects ſuſceptible of this form of fever. About the middle of the month it again became ſultry; rains were frequent, and the heat of the ſun oppreſſive; and although the number of

new cases was very sensibly decreased, yet no vessel arrived from Europe, or the northern States, without some of the crew being immediately affected with the prevailing complaint. Towards the latter end of September the fever appeared under a new form in young subjects of both sexes, but more particularly with delicate females; many young persons, who had hitherto escaped, were now overtaken with the disease, in the well marked form of typhus gravior: free from local complaints they exhibited marks of strong nervous affection, and generally became deeply comatose three or four days before death.

On the 5th of October a deluge of rain fell, accompanied with a powerful sweeping wind from the north-east; the weather became suddenly very cold; the mercury fell to 48 degrees on the morning of the 6th, and on the 7th it was as low as 42 degrees of Fahrenheit. In a few days after this, not a vestige of the Yellow Fever was to be seen in Norfolk; but children under 14 years of age were very commonly attacked with a fever of a continued type, with severe nervous affections, and frequent discharge of worms. The weather became hot again on the 12th and 13th (on the latter day the mercury stood at 80 degrees in the shade, and it was oppressively hot in the sun) and continued till about the 20th. The cessation of the fever in the early part of this month induced those who had removed to return, and strangers began to visit us after the 15th; several of whom were attacked with this fatal disease, and not being aware of its existence, neglected themselves in the commencement, and, in many instances, fell victims to it; but since the 30th all descriptions of persons have enjoyed uninterrupted good health. Of the mortality caused by the fever no accurate register has been kept, but from the most careful investigation of this subject that now can be made, we believe the number that died this year of the Yellow Fever, not to have exceeded 250.

It is unnecessary to enter into any particular detail of the medical treatment that was followed in combating this formidable disease. It will be sufficient to observe, that the depleting plan of cure was adopted in its fullest extent, and the antiphlogistic regimen, as far as circumstances would permit, was strictly adhered to. Mercury was exhibited liberally, and we found, as has been observed by other practitioners, that where a free salivation was brought on, the patient uniformly recovered. But whether we ought to regard this effect of the mercury as a proof of the energies of the constitution not sub-

dued by the diſeaſe, or as a legal victory obtained over the fever by means of the remedy, is what we have not yet, in our minds, been able to determine. Bark, wine, and cordials, in the early ſtage of the diſeaſe, were found always to do harm. The affuſion of cold water, after the bowels were duly emptied, was alſo had recourſe to with decided advantage. In ſeveral caſes where the cure of the diſeaſe was intruſted chiefly to cold bathing, it ſeemed to yield readily to that remedy; but it muſt be owned that theſe caſes were not of the moſt violent kind, and the remedy was applied on the very firſt attack. Although we have no reaſon to believe that the ſeveral remedies employed in the caſe of this diſeaſe have been leſs ſucceſsful in our hands than in thoſe of others, yet we are inclined to think that much remains to be learned both as to the nature and cure of the Yellow Fever. In this opinion we agree with an ingenious gentleman, lately much employed in the treatment of this complaint in the Iſland of St. Domingo: he ſays, "Quicunque medicinæ ſcientiâ confiſus, jactat, ſe hunc morbum poſſe certò curare, vel ſeipſum decipit, vel vult alios decipere."

ARTICLE II.

CASE *of* TETANUS *from a* PUNCTURED WOUND *in the* HEEL *cured by the* TINCTURE *of* CANTHARIDES: *Communicated in a Letter from Dr.* SAMUEL BROWN, *of Lexington (Kentucky), to Dr.* MITCHILL, *dated Dec.* 1, 1800.

ON Wedneſday, the 28th of October, 1800, I viſited Miſs P— D—, a young lady aged nineteen. On Monday night, previous to my viſit, ſhe, unfortunately, ſet her foot upon a nail, which, entering the middle of the heel, penetrated quite to the os calcis. At firſt ſhe complained of a numbneſs rather than pain; but in the courſe of half an hour the pain became tormenting in the wound, from which, however, it ſoon removed to the ſides of the ancle, leaving the puncture perfectly free from any uneaſineſs. During the night ſhe continued reſtleſs, though the pain, at intervals, conſiderably abated. On Tueſday evening all the ſymptoms became more violent. The wound diſcharged nothing except a drop or two of blood and ſerum; the pain became more acute in the ancle, and extended to the knee, hip and joints of

the back. In the night ſhe had many twitches of the nerves, and ſpaſms of the muſcles of the arms, and thoſe which retract the head. Before the morning there was ſo great a degree of rigidity in the back of the neck, that the relations were at no loſs with regard to the nature of her complaint. A phyſician was ſent for, who unluckily, was at a horſe-race, and declined to viſit her. He, however, ſent a few anodyne pills, which ſhe took without any material advantage. It was twelve o'clock on Wedneſday night when I arrived. I found the patient in extreme agony, with ſpaſms in her back and neck, and acute pains in her ancle, knee, and hip joint. On examining the puncture, not the ſlighteſt appearance of inflammation could be diſcerned; the heel was preternaturally cold, the ſkin was pale, and nothing but blood and ſerum could be preſſed from the wound. I introduced my probe, and was greatly ſurpriſed to find that ſhe was ſcarcely ſenſible of pain from a very harſh examination of the direction and extent of the puncture. At length, however, the point of my probe touched a nerve or ligament, which appeared to poſſeſs extreme ſenſibility. Having fixed the probe firmly againſt the os calcis, I took a ſcalpel, and cut down the ſide of the probe through the ligaments quite to the bone, in a tranſverſe direction with regard to the foot. Immediately on making this inciſion, the whole of her complaints ſeemed to be removed, and I flattered myſelf, that I ſhould be able, by the uſual remedies, to prevent a recurrence of the ſpaſms. But, anxious more effectually to deſtroy the injured nerve, and to excite inflammation in the part, I waſhed the wound with ſpirits of turpentine; and, having made a doſſil of lint, I covered it with powdered ſulphate of copper and ſpirits of turpentine, and preſſed it down to the bottom of the inciſion. This application gave ſevere pain; but the patient aſſuring me, that it was much more tolerable than the ſpaſms, I ſuffered it to remain. I now gave her a very large doſe of laudanum, which I directed to be repeated every four hours—I likewiſe laid a large bliſter on the ancle, where the pain had been always very diſtreſſing ſome time before it extended to the body. Warm poultices were kept to the heel. I ordered two grains of calomel every hour when ſhe was awake, and had ℥i of mercurial ointment rubbed into the legs, arms, and back—I likewiſe endeavoured to haſten ſalivation, by rubbing calomel on the gums. She ſlept about two hours, and continued tolerably eaſy until twelve or one o'clock on Thurſday, when her ſpaſms returned—I was again ſent for, and arrived about dark. All the ſymptoms were then

greatly aggravated; the ſpaſms more violent than I had ever ſeen them; they were more general—her jaws were ſtiff, her deglutition difficult, and the ſtridor dentium ſo great as to be heard in a very diſtinct manner all over the houſe. Recollecting the caſes which occurred to Mr. Dallas, and his friends, in the iſland of Carciacow, as publiſhed in the Edinburgh Medical Annals, from which, it appears, that not one perſon in fifty who is affected with tetanic ſymptoms, from the læſion of a nerve, recovered, notwithſtanding the moſt approved modern remedies were vigorouſly employed, I reſolved to give my patient a chance for life, by attempting to relieve her by a new mode of treatment.

The caſe appeared ſo hopeleſs, and death ſo near, that I thought the "*anceps remedium*" perfectly juſtifiable. Having, in vain, endeavoured to ſubdue the ſpaſms, by antiſpaſmodics, to produce ſalivation by the liberal exhibition of mercury, and to excite inflammation in the wound by the ſtrongeſt ſtimulants, I determined to make trial of wine and bark, which had proved ſo efficacious in the hands of Dr. Ruſh. Theſe were immediately rejected by the ſtomach. The bliſter, after remaining on the ancle, from Wedneſday until Friday night, produced very little effect on the ſkin.

By this time ℥ ij of mercurial ointment had been rubbed in, xxij grains of calomel taken in form of pills, and ℈i. rubbed on the gums. I now deſpaired of being able to excite ptyaliſm early enough to prevent the fatal termination of the diſeaſe. I therefore directed fifteen drops of the Tinctura Cantharid. in a cup of tea, to be taken every hour, until the ſymptoms of inflammation in the bowels forbid their further exhibition. I now removed the dreſſings from the wound, and filled it with the root of the phytolacca decandra (or poke-root), which produced very great uneaſineſs for two or three hours. The hot poultices were continued, together with the uſe of injections, which had been found neceſſary during the whole courſe of the diſorder.

After the patient had taken ʒ iſs of the tincture of cantharides, ſhe was ſeized with a ſenſation of heat and burning in the ſtomach, which, becoming more and more ſevere, extended through the whole courſe of the alimentary canal, and produced a number of ſtools, mixed with blood and mucus. The inflammation of the ſtomach occaſioned vomiting, and the acute pains in her bowels reſembled thoſe which are felt in the moſt violent attacks of dyſentery. *Immediately on the occurrence of theſe ſymptoms, all the tetanic affections diſappeared, and*

never returned. In order to mitigate the diſeaſe which I had produced in the alimentary canal, I gave large quantities of mucilaginous drinks, and anodyne enemata, which afforded conſiderable eaſe. Fortunately, however, the day after this inflammatory ſtate of the bowels came on, the mercury, which before ſeemed quite inert, began to ſhow its effect on the ſalivary glands, and (as we have often ſeen in dyſentery) removed the diſeaſe of the inteſtines. Some ſlight affection of the kidneys appeared, with bloody urine, but ſoon left the patient without any complaint, except a very moderate ſalivation, which continued for about two weeks. The wound began to form pus eight days after the accident, and was kept open ſeveral weeks, from an apprehenſion, that if it cloſed, the ſpaſms might return.

This caſe, which terminated ſo happily, is not communicated to the public as the foundation of any new favourite theory. The writer is ſufficiently aware of the danger of deducing general inferences from ſolitary facts. Diſappointed in his expectations of relieving his patient by the faſhionable remedies, his practice was the reſult of that neceſſity which occaſionally gives birth to the moſt uſeful diſcoveries. He thought it his duty to communicate it with ſimplicity and candour, and hopes that thoſe who practiſe medicine in climates where tetanus is a common diſeaſe, will ſtrive to leſſen its mortality, by giving cantharides a fair trial. It is well known that phyſicians are often under the neceſſity of combating one diſeaſe by creating another, and although it muſt be confeſſed that the inflammation of the ſtomach and bowels is attended with great danger, no practitioner will heſitate to affirm that tetanus is ſtill more dangerous.

ARTICLE III.

On the CONNECTION *of* EARTHQUAKES *with* EPIDEMIC DISEASES, *and on the* SUCCESSION *of* EPIDEMICS: *Communicated in a Letter from* NOAH WEBSTER, jun. *Eſq. to* Dr. MILLER, *dated New-Haven, Feb.* 21, 1801.

IN the courſe of reading, I frequently find accounts of epidemic or malignant diſeaſes, which corroborate, in a ſurpriſing degree, the opinions I have advanced in my hiſtory. Some of theſe accounts I will occaſionally communicate to you, and you will judge whether the facts are worthy of a place in your Repoſitory.

In the year 1770, Hiſpaniola experienced numerous ſucceſſive ſhocks of earthquake. For many months, ſcarcely a day paſſed without a ſhock of greater or leſs violence; nor was the following year exempt from them. During theſe convulſions, a malignant diſeaſe broke out at Leogane, Port au Prince, and other places near the Bite, which muſt have been the real plague; for, ſoon after the patient was ſeized, a bile or tumour appeared under the arm, and he died in about eighteen hours. The Commander in Chief retired from that part of the iſland, all commerce with thoſe ports was voluntarily abandoned for ſome months, and troops were poſted to prevent all intercourſe between them and the neighbouring country.—This diſeaſe was, at the time, aſcribed to the noxious vapours ejected by the convulſions of the earth and eruptions. Theſe facts are extracted from various gazette-accounts of the years mentioned.

"In 1770," ſays Moſeley on tropical diſeaſes, p. 44, London edition, 1795, "the nervous remittent fever, the endemic of Jamaica, entirely changed its type. It took on an immenſe miliary eruption, and other circumſtances never before obſerved. It raged with violence, and killed almoſt all the young people who were ſeized. It returned the two next ſprings, and then diſappeared." I ſhall only obſerve, that this change in the type of that diſeaſe was cotemporary with the events before related in Hiſpaniola, which is in the neighbourhood of Jamaica.

In the ſame year, a petechial fever broke out at Hartford, in Connecticut, in the family of one Burnham; moſt of the family, and ſeveral others died. This account I have from a friend of mine, who was then affected with the diſeaſe, and from an attending phyſician.

On the 7th of January, 1780, a ſevere earthquake was felt at Jamaica. In April following, a malignant ulcerated ſore throat appeared, and killed almoſt every child ſeized. It abated in ſummer, but attacked adults in September. It was very inſidious in its attacks; frequently the whole throat, fauces, and palate were rotten before the diſeaſe was perceived. In October following appeared the malignant yellow fever. Moſeley, p. 182, 245.

I have related theſe laſt facts only for the ſake of exhibiting their order—for it is the ſame as in this country—the malignant ſore throat frequently, not to ſay uſually, preceding the malignant yellow fever and dyſentery. The moſt deadly dyſentery that prevailed in Connecticut, the laſt half century,

was in 1751 and 1773—in both years preceded by the angina maligna in ſpring; as was the yellow fever and dyſentery of 1794.

To the facts illuſtrative of the effects of earthquakes on the human body let me add, that the great earthquake, which deſtroyed Liſbon, in 1755, was followed by a diſtemper ſo fatal as to raiſe an alarm in England, and an order was iſſued by government, for all ſhips from Portugal to ride quarantine, notwithſtanding the calcareous baſis of Liſbon. My authority for this fact is not before me, but it is the Gentleman's Magazine for that or the following year, and may be ſeen in the New-York Library. In proof of the ſame point I can heap Pelion upon Oſſa,* but it is not neceſſary, as I believe the point to be conceded by medical gentlemen.

Another point ſuggeſted in my hiſtory, and on which no ſmall ſtreſs is laid, is, that the moſt generally prevalent, and moſt fatal epidemics, are every where introduced by the influenza. I apprehend the fact to be proved, that the influenza in Egypt, and the tropical regions of America, is invariably and ſpeedily followed by peſtilential diſeaſes. In addition to the maſs of evidence accumulated in my hiſtory, pleaſe to accept the following facts:

In the winter of 1772—3, and immediately after the influenza of that period, a malignant diſeaſe appeared in Mexico, and the adjacent country. By the Spaniards it was called the plague. Twenty thouſand perſons died of it in a few days. How long it continued is not mentioned. This account is from a public gazette, inſerted as a letter from Spain. It will be recollected, that in North-America the influenza of the ſame period was followed by meaſles, angina maligna, and dyſentery. In North-Carolina it was followed by an epidemic

* This alluſion may be thought, by ſome, not claſſically correct; for Virgil deſcribes the giants, in their war againſt Jupiter, as attempting to place OSSA UPON PELION:

> Ter ſunt conati imponere Pelio Oſſam
> Scilicet, atque Oſſæ frondoſum involvere Olympum. GEORG. I.

But Longinus, it ought to be remembered, (Περι Ὑψους §. 8.) has preſerved a quoted paſſage, by which it appears, the other mode of piling was undertaken: and yet theſe ſons of Titan, who, on the Phlegrean plains of Theſſaly, ſtrove to heap the mountains on each other, did not ſucceed in their enterpriſe.

The circumſtances under which ſickneſs from ſeptic effluvia *may* occur, notwithſtanding the prevalence of *lime-ſtone*, *chalk*, and *other kinds of calcareous earth*, were ſtated and explained as long ago as 1797. See Medical Repoſitory, vol. ii. No. 1, art. x. EDIT.

bilious fever in autumn 1773, unuſually violent. In July of that year, Guatimala, in the Mexican country, was ſunk by an earthquake, and 30,000 people periſhed. The ſame ſummer was very ſickly at New-Providence; but New Spain, and the Bahamas are ſaid to abound with lime-ſtone.

The ſcarlatina anginoſa appeared in the eaſtern part of Connecticut, in Providence, and Newport, in the winter of 1783—4. It prevailed in Lyme, a town in Connecticut, lying on the river and the Sound, and which contains ſome ſalt marſh, but, in ordinary years, is as healthful as other towns in the State. In the ſummer of 1784, an epidemic bilious fever ſeized the inhabitants, to an extent not before recollected by elderly men. This is the more remarkable, as the ſcarlatina of that period fell lightly on the weſtern parts of this State; but was more ſevere in the eaſtern towns, and in Maſſachuſetts, New-Hampſhire, and Rhode-Iſland. The connection in the epidemics at Lyme is not to be paſſed without notice. My authority is Dr. Mather, of that town.

I find an account of a very fatal bilious plague in Barbadoes, in the winter of 1647—8, introduced by the influenza of 1646, which I have tranſcribed and ſent to the academy of medicine. It is the firſt inſtance, I believe, on record, as Barbadoes had then been ſettled but about twenty years. This was long prior to the period aſſigned to the importation of the Siam fever. Had the writers on that diſeaſe, whether French or Engliſh, examined the hiſtories of America, the idea of its introduction from Siam or Africa, would never have entered their brains. Siam and Africa are as exempt from that malignant fever, in ordinary years, as Barbadoes or New-York.

Let it not be overlooked, that in Barbadoes, the ſame diſeaſe was univerſally ſevere *after* the influenza of 1733, 1737, 1761—2. I wiſh we had more light on this ſubject, and that medical gentlemen in the iſlands would examine the hiſtory of the diſeaſes, as well as the memories of old people for facts. Barbadoes is ſaid to have a baſis of lime-ſtone, and this, as well as all the iſlands, are healthy in ordinary years.

In my hiſtory, vol. i. p. 251, I have mentioned the extreme drouth of 1761 and 1762, and have ſuggeſted, from the concurrent evidence of a great number of facts, the probability that an extraordinary evaporation, as well as failure of rain, precedes and accompanies eruptions of fire. I have ſince found an account of a remarkable eruption in Mexico, during that drouth. At Juruyo, in Michuacan, was only a ſmall hill in 1760, on which was a ſugar plantation. On the 29th of

September of that year the earth burſt open, with furious ſhocks, and continued to emit fire and burning rocks for ſome years, until the maſs ejected formed three high mountains of ſix miles in circumference. The aſhes were wafted 150 miles, and, at 60 miles diſtance, covered towns, ſo as to oblige the people to ſweep their yards two or three times in a day. See Clavigero, vol. i. 14. Note. This is an intereſting fact, as it throws light on the formation of volcanic mountains, and is one of the moſt recent inſtances. I do not, you will obſerve, undertake to allege, that the ſevere drouth of that period had a connection with the eruption, but the coincidences of this kind are remarkable and deſerve notice.

ARTICLE IV.

FACTS *concerning the* NOXIOUSNESS *of* RAIN-WATER *and* DEW *in* 1797: *Communicated in a Letter from Dr.* FOOT *to Dr.* MILLER, *dated Greenſburg, Dec.* 29, 1800.

IT is with pleaſure that I comply with your requeſt, to furniſh you with the facts concerning the morbid effects of rain-water in this quarter of the country, in the ſummer of 1797. As I was not a reſident within this State at that time, and, of courſe, not a perſonal obſerver, I have delayed compliance until I could inform myſelf of more particulars, of the extent, duration, and other circumſtances of the occurrence.

After making particular inquiry, I find but few perſons of common curioſity, of thoſe who had been in a condition for this kind of obſervation, who had not noticed theſe uncommon appearances; and when I conſider the publicity and great ſingularity of the facts, conjoined with their importance, in ſupport of certain phyſical principles, and the intenſe thirſt for philoſophical reſearches, which characterizes the preſent age, I have been ſurpriſed that they have not heretofore found way to your Repoſitory; a work which I cannot but conſider as the proper receptacle for every thing novel or intereſting in the philoſophical world, and which, for the intereſts of medicine, it is to be regretted, had not been able to boaſt of an earlier origin in this country. Facts, of ſufficient importance, from which to infer new principles, or even to ſupport analogies, are not of daily occurrence, and may not be repeated in the ſame country in ſo intereſting a manner in an age: conſider-

ations which render a work like yours eſſentially important, and which ought to operate as a powerful ſtimulus to country practitioners of medicine within the United States, whoſe ſphere of obſervation, it muſt be acknowledged, is comparatively great.

Since the late improvements in Chemical ſcience, and the writings of Dr. DARWIN (who, however, in ſome of his principles, was anticipated by the ingenious BROWN), I believe, that the identity of thoſe laws which govern animal and vegetable bodies is pretty generally acknowledged. This is a diſcovery of incalculable importance to the intereſts of medicine. It has preſented to the phyſician an extenſive field for improvement by analogical reaſoning, and has called to the aid of medicine almoſt every other ſcience. For amuſement, the phyſician ſtands on a level with the Botaniſt and Chemiſt; and, in ſhort, the healing art, inſtead of a mere collection of ſymptoms and preſcriptions, incoherent, and frequently contradictory, has become reducible to calculation, and merits the title of a ſcience, affording the ingenious ſtudent a field for amuſement, as engaging as it is extenſive.

Craving your indulgence for the preceding, and I had almoſt ſaid involuntary obſervations, the facts of which I promiſed you a relation were as follows:—The latter part of the ſummer of 1797, was rendered remarkable, by the fall of an uncommon quantity of rain. It commenced about the 15th or 16th of July, and continued, with ſhort intermiſſions, until paſt the middle of the ſucceeding month. The earth, by this means, became ſo loaded with water, that it was almoſt impoſſible to walk with dry feet, even over the moſt elevated fields. The conſequence was, in this quarter of the country, a great loſs of property experienced in the damaging of much hay, and in the proſtration of ſome valuable mill-dams, with their mills.

But the greateſt ſingularity attending this ſeaſon was manifeſted in the inconvenience reſulting from the effects of this water on the bodies of that claſs of people (farmers) who were neceſſarily moſt expoſed to its influence. This appeared in an uncommon and troubleſome ſoreneſs of the hands and feet of thoſe, particularly, who were occupied in ſaving their hay from the rain in low-land meadows. An erythematous inflammation of the whole part immerſed in water, frequently took place on the ſame day; and, in addition to this, often a complete abraſion of the cuticle of the whole foot and leg, in many attended with ſuch a degree of pain and tumefaction as to lay the ſubject by. In others, beſides the above effect, there frequently aroſe on the part, more eſpecially between and about

the toes, where the water would be longeſt retained, a livid-coloured veſicle, which, on breaking, diſcharged a dark grumous-like matter; this left behind an ulcer of the phagedænick ſpecies, which became difficult to heal, and continued troubleſome even for weeks. But the moſt ſtriking proof of acrimony in this water was, that a ſwelling and ſoreneſs (as I am credibly informed) took place, in ſome inſtances, on the ſame day, after ſlightly wetting the feet, in walking through the dewy graſs of uplands in the morning. Theſe appearances were generally noticed as ſingular; yet few ſuſpected them the reſult of any morbid quality in the water; and as one of the moſt ſtriking (though not leſs miſchievous) propenſities of human nature is, that in caſe the real cauſes of effects do not readily preſent, to impute them, however ſingular, to the moſt obvious, ſo the conjectures on this occaſion were various, vague, and very unſatisfactory. Whether any ſingularity in this water was diſcernible by the organs of taſte, I have not been able certainly to learn. On aſking an old gentleman, a neighbour of mine, whether he taſted of this water, he replied, with a degree of engagedneſs, "that, when at labour from his houſe he had occaſion to take a draught from a ſmall rill iſſuing from the ground, it affected him ſo ſenſibly, that he did not forget it in a number of days." Inſtances of this kind may have been more frequent, but as the quality of water is materially changed by filtration through the earth, it is probable they were not numerous.

I have not likewiſe been able to ſatisfy myſelf, whether the health of other animals was materially affected; certain, however, it is, that ſwine, at this time, and often, were attacked with a diſeaſe, which proved, among them, unuſually mortal. The ſymptoms were, a loſs of appetite, and conſequent emaciation, attended with a diſcharge from the inner angle of the eye of a frothy limpid fluid, producing a depilation, and even excoriation of the whole cheek in its courſe. To what cauſe this ſingular complaint among that ſpecies of animals is imputable I ſhall not conjecture. The remedies which proved of moſt efficacy in ſaving them, I am informed, was a free exhibition of brimſtone combined with their food. If ſuch were the effects of theſe waters on animal bodies, we ſhould expect them to be at leaſt not leſs conſpicuous on vegetables: theſe appeared particularly in the deſtruction of tender garden plants, ſuch as cucumbers, &c. and in a diſcolouration of the graſs where the water remained any time, and in its complete deſtruction in patches about meadows, as effectually as if ſcorched by fire.

I have been more particular in a relation of these appearances, as they took place some time past. With respect to their extent, I believe, from the best information I have been able to obtain, that they were limited by the south of this, but extended northwardly quite into Dutchess county, easterly to the distance of a few miles only. The circumstance of limitation seems to involve some obscurity. Neither have I been able to learn at what period of this rainy season these effects were most conspicuous.

I am not qualified to hazard any conjecture on this subject. You will recollect that the summer of 1797 was rendered remarkable by numerous occurrences, which go strongly to establish the doctrine of ocult causes in the atmosphere—a doctrine which contains many valuable arcana, and from a developement of which important acquisitions will result to the science of medicine.

Did these waters possess an acrimonious quality previously to their precipitation? If so, was it of volcanic origin? Was the atmosphere of that season compounded of more than its usual quantity of azote? Or did the morbid properties of this water arise from its meeting with some agent on the surface of the earth? If so, was this septic acid simply from vegetable decomposition, aided by the water as a menstruum? And did this water possess any unusual quality which favoured this decomposition?

ARTICLE V.

Case of ALKOHOL *administered to a Man after having taken an immoderate Dose of* OPIUM: *Communicated to Dr.* MITCHILL *by Mr.* SHEPHARD, *in a Letter dated New-York, Dec.* 17, 1800.

I SUBMIT to your consideration a case of debility, arising from the effects of Opium, that fell under my care, which, perhaps, more accurately illustrates the operation and effects of *this* medicine, and shows to what extent *it* may be exhibited, and the patients still have a chance for recovery, than almost any experiment by which it has been preceded.

Daniel Grayham, aged about 35 years, of a firm, robust habit, and apparently in perfect health, residing at the time transiently in Schenectady, in this State, took the fatal resolu-

tion of deſtroying himſelf, and made the deſperate attempt by the means of opium. Early on the morning of February 4, 1800, he procured, of a phyſician in that town, two drachms of opium; and then, repairing to an adjacent inn, called for a bed (complaining of illneſs), and immediately took the intended fatal portion. This, as nearly as could be aſcertained, was about eight o'clock in the morning; and about eleven he was diſcovered by one of the family. I ſaw him immediately, and found him in a ſtate of inſenſibility, accompanied with a complete apoplectic reſpiration, and ſome ſlight convulſive contractions of the muſcles, eſpecially of the hands and face. The pulſe was full, regular, and not perceptibly removed from the ſtandard of health. From theſe appearances I ſuppoſed him to be in the interval of an epileptic paroxyſm; it not being known, at this time, that he had taken any thing to injure himſelf, or even had the deſign in contemplation. The ſymptoms, however, continuing the ſame for a conſiderable length of time, without any ſucceſſion of ſpaſm, or appearance of returning animation, I was totally at a loſs how to proceed, or, in fact, what opinion to form of the diſeaſe. Some other phyſicians were now called in by the general alarm; but, previous to their arrival, I had accidentally diſcovered a paper upon the floor, in which, I found, had been opium; and which, at once, developed the wretched ſcene before me.

One of the phyſicians who now attended propoſed, on this diſcovery, to immediately give an emetic, which was accordingly exhibited: but as the opium had had its full effect long before, no benefit could be expected from *ſuch* practice. I exerted myſelf to have an oppoſite mode of practice purſued, and could not effect my purpoſe; but calmly contented myſelf with looking on, while a variety of other exertions were made, without the leaſt advantage.

As a dernier reſort, he was, about two o'clock P. M. placed into the hot bath, in which he was immerſed a conſiderable, and, probably, a ſufficient length of time; but the reſult of this experiment was ſimilar to every former trial; and he was now abandoned by all to the certainty of his fate.

For ſome time previous to this period I had been abſent; but about four o'clock, and near two hours after all exertions in the patient's favour had been diſcontinued, I again ſaw him.

To ſupport the ſyſtem by the exhibition of an *adequate* degree of ſtimuli was the object I had before wiſhed to pur-

ſue. In *this* laſt ſtage of the diſeaſe it was the *only* object. And although I ſcarcely retained a hope of ſucceſs, yet the dictates of humanity, the proſpect of future utility to the world, and the confidence I had in the principles on which I proceeded, urged me not to relinquiſh every hope while exiſtence remained. *This* was the moment to which the ſtimulant mode of cure was peculiarly *adapted*; for now the effects of the opium were ſubſiding, and the ſyſtem rapidly ſinking into the impending diſſolution with which it was momentarily threatened. The apoplectic reſpiration now nearly ſuſpended, and a weak, ſinking, tremulous and irregular pulſe, were the only indications of remaining vitality. His ſkin was univerſally cold, and partially ſuffuſed with a glutinous moiſture; and his cadaverous countenance was literally the aſpect of death. The tongue and mouth (which were widely extended) were perfectly dry, and produced, to the touch, a ſenſation ſimilar to that ariſing from the action of a raſp; and, in fact, every appearance pointed to an immediate termination of the tragical ſcene. Theſe were the formidable ſymptoms I had to encounter; and, as the patient was expoſed to public view, I ſuſtained a degree of cenſure for the temerity of retaining a hope of ſucceſs in a ſituation ſo deſperate. The ſafety of the patient, and my own reputation, were ſources of great anxiety; but ſtill I adhered to my firſt reſolution of trying the experiment.

I took ſome brandy, which having ſweetened, and diluted with a trifle of water, I exhibited a little immediately after each reſpiration, the intervals of which were now near a minute. I ſoon found the mouth and tongue were aſſuming a degree of natural ſoftneſs, and was confident the liquid was gradually, though quite mechanically, paſſing into the ſtomach; and in this cautious manner I very ſoon gave more than a gill of the brandy; and I flattered myſelf I diſcovered ſome ſlight ſymptoms of returning animation. At this period I was obliged to leave the patient with an attendant, directing him, with the moſt forcible injunctions, to ſteadily purſue the plan I had adopted. I was abſent near an hour; and, on my return, had the pleaſing ſatisfaction of finding my patient perfectly ſenſible, able to turn and raiſe himſelf in bed, and to converſe freely; but incapable of ſupporting an erect poſition, in conſequence of the vertigo incident to that ſituation, in caſes of extreme debility.

He had now taken about one pint of brandy. His irritability was ſo great, that the ſmalleſt cauſe of emotion would

excite great, and ſometimes an alarming degree of agitation. I continued this practice through the night, without any variation, excepting gradually diminiſhing the quantity of ſtimulus exhibited. The next morning I changed the ſtimulus by alternating the brandy with wine; which I continued through the day and night enſuing, ſtill diminiſhing the quantity of ſpirits; and, as ſoon as the ſtomach would admit, gave him biſcuit, and wine and water, and from thence proceeded to the uſe of freſh-meat broths. On Thurſday he took a portion of calomel; which, I preſume, from the irritable ſtate of the ſtomach, proved violently emetic, and very ſlightly cathartic.

This medicine was materially ſerviceable, as his lungs had been all along ſomewhat oppreſſed; which, by this, was relieved, and the ſtomach and bowels ſufficiently cleanſed for the reception of the bark. From this period the bark was exhibited, and the laudanum and wine uſed as ſtimuli; and the caſe, in every reſpect, treated as a convaleſcent of indirect debility. In about ten days he was able to leave the town, which he performed on foot.

After his recovery he informed me, that his deſign being to make his exit ſure, he had taken all the opium procured of the phyſician above-mentioned, which he ſaid was *two drachms* (the truth of which the phyſician afterwards confirmed to me), together with a piece he had before obtained, as he ſuppoſed about half a drachm; and that he retained his recollection for about ten minutes after taking the doſe.

One obſervation occurred to me, which, I preſume, will be found of conſequence in the treatment of ſimilar caſes; which is, that *no* ſtimuli ſhould be exhibited till the effects of the one conſtituting the diſeaſe are ſubſiding, and the ſyſtem diſcovers evident marks of ſinking. This is the period to employ the aſſiſtance of other ſtimuli, to give due ſupport to the habit, and to excite and maintain *its* action till the morbid diſturbance has *ceaſed to be*. Had the brandy been given on the firſt diſcovery of the patient, undoubtedly the already too highly excited action would have been increaſed, the hopes founded on this practice fruſtrated, and the patient loſt. There is room ſufficient for obſervations, and for a good deal of theoretical ſpeculation, on a caſe ſo ſingular as the above: but, as I was under the neceſſity of protracting it to the length in which it now appears, I preſume that further remarks may be diſpenſed with.

ARTICLE VI.

OPINION *of the* MEDICAL FACULTY, *addreſſed to* JAMES CALHOUN, *Eſq. Mayor of the City of Baltimore, relative to the* DOMESTIC ORIGIN *of the* PESTILENTIAL SICKNESS *in that City, during the hot Seaſon of* 1800: *Communicated by Dr.* DANIEL MOORES *to Dr.* MITCHILL.

SIR,

IN compliance with your requeſt, the Medical Faculty of the city of Baltimore have taken into conſideration the ſubject of the late epidemic, and herewith tranſmit to you the reſult of their inveſtigation of its origin, together with their opinions reſpecting the moſt probable means of obviating the recurrence of a ſimilar calamity.

After the moſt ſcrutinizing inveſtigation, the faculty have found no proof, or even cauſe of ſuſpicion, that the fever which lately ſo unhappily afflicted our city was derived from foreign cauſes. The following conſiderations, they are perſuaded, will tend to elucidate this opinion.

I. The prevalence of an *epidemic conſtitution of the air*, which, although not yet demonſtrated by chemical analyſis, is ſufficiently apparent from the following effects:

1. It appears, from the united teſtimony of the faculty of medicine, throughout the United States, that the diſeaſes of our country have for late years, manifeſted a more malignant character. Thoſe places formerly ſubject to mild intermitting and remitting fevers, have lately been viſited by diſeaſes of a more inflammatory diatheſis. In the weſtern part of this State, a fever of a very malignant type has prevailed for two or three years, very different from any diſeaſe hitherto known in that tract of country.

2. The morbid diſpoſition of our atmoſphere has been further evinced by its deleterious influence on certain brute animals in various parts of the United States; it has affected horſes, horned cattle, dogs, cats and poultry. Now the general hiſtory of epidemics proves that this affection of certain brute animals, either precedes or accompanies their prevalence.

3. The exiſtence of ſuch a ſtate of atmoſphere diſcovers itſelf to our ſenſes, by the various ſenſations which are perceived during the prevalence of that fever, even by thoſe not

confined with it, more eſpecially in perſons not accuſtomed to inhale the impure air of our city. The hiſtory of all countries proves, that great epidemics have viſited them at ſome period, and it would be extraordinary indeed, that the United States ſhould be the only country on the globe exempt from their invaſion.

II. That the late epidemic fever was derived from domeſtic ſources we think the following facts and circumſtances will ſufficiently demonſtrate. The firſt perſons who ſickened had no communication with veſſels engaged in foreign commerce, but were expoſed to powerful local cauſes, and were attacked at ſuch diſtances from each other, as to preclude the probability of any one of them having derived it from the other.

The caſe of John Harris, viſited by Dr. Rounald, near Jalland's garden, Fell's Point, on the 2d of May; Thomas Sherwood, viſited by Dr. Potter on the 2d of May—theſe two caſes occurred immediately ſucceeding the remarkable warm weather during the laſt week in April; Captain Conner on the 8th of June, Mrs. Davis on the 9th, John Mills on the 13th, Captain Derochbroom's apprentice boy on the 22d, Captain Stewart's niece on the 24th, Baſit Gale's ſiſter on the 24th, and ſeveral others, all viſited by Doctor Coulter previous to the 1ſt of July; James White, viſited by Doctor Allander on the 10th of June; Mrs. Wilſon, viſited by Doctor Dunhel on the 21ſt of June; Jonah Thomas (John-ſtreet, Old Town), viſited by Doctor Archer on the 28th of June; Thomas Ing, near the corner of Gay and Frederick ſtreets, viſited by Doctor Archer on the 3d of July; Thomas Sorin, about fifty yards from Jonah Thomas's, viſited by Doctor Archer on the 25th of July. The above caſes attended by Doctor Coulter were at Fell's Point, where they had been expoſed to powerful local cauſes, emanating from the cove hereafter to be deſcribed, and to other ſources of peſtilential exhalation. Thomas Sherwood had worked near a putrid ſink of water, containing vegetable matters, in a ſtate of putrefaction, and the offenſive condition of Jonah Thomas's market yard, and the gutters and ſtreets near his houſe, are well known to have annoyed all who paſſed them. We have in vain endeavoured to trace any of thoſe caſes to veſſels employed in foreign commerce; none of them had any communication with any known vehicle of human contagion. Theſe caſes and ſome others occurred before the fever became general, and did not communicate to any perſon; if it had been derived from human contagion, why did they not communicate it in

the ſame manner as it muſt have been communicated to them? It is no inconſiderable argument in favour of our opinion to remark, that a ſimilar diſeaſe has viſited many of the interior parts of the United States, remote from all communication with the Atlantic, but where the local cauſes were too evident to eſcape the notice of the moſt ſuperficial obſerver. Not to enumerate many of the places where a diſeaſe has been marked with all the characteriſtic ſymptoms of the yellow fever, and with the ſame mortality as in the city of Baltimore, we would aſk, if the diſeaſe was imported, how it could have reached Galliopolis on the Ohio, Mifflin County in Pennſylvania, or the Geneſee Lakes in the State of New-York? To ſuppoſe that the fever which has ravaged theſe new ſettlements at ſuch a diſtance from the Atlantic, could be derived from the Weſt-Indies, would be too unphiloſophical to merit attention.

III. The general extinction of this diſeaſe by froſt, proves that the cauſe exiſts in the atmoſphere, for fevers ariſing from human contagion are admitted to be moſt contagious in cold weather.

IV. Is it not very extraordinary that we ſhould be ſuppoſed to receive a diſeaſe by imported contagion, which is only occaſional in the country whence it is allowed to be derived, and is there ſcarcely thought contagious?

V. We are, moreover, confirmed in our opinion of the indigenous origin of the late epidemic fever, by having learned that a ſimilar diſeaſe prevailed, at the ſame time, in many other parts of this State; viz. in Harford County, on the banks of the Suſquehanna—on the canal in Cecil County—in Charles County—in Dorcheſter County—and in ſome other places.

VI. The reaſons, in our opinion, why this diſeaſe has been ſo long and ſo erroneouſly aſcribed to imported human contagion, is the circumſtance of its having ſo often originated on ſhipboard, from foul air, generated by improper ballaſt, or other ſubſtances in a ſtate of putrefaction. Indeed, the confined ſituation of the holds of veſſels is, of all circumſtances, the moſt likely to be productive of noxious effluvia. The hiſtory of the yellow fever which originated on board of the United States frigate General Greene, affords a melancholy illuſtration of this opinion. The narrative given by Dr. Kollock ſtates, that this ſhip ſailed, on her firſt cruiſe, from Rhode-Iſland, and that the yellow fever appeared on the 15th day from the time ſhe left Newport, and is aſcribed, by him, to her having ſprung a leak, and thereby affected her ballaſt,

which was compoſed of iron, ſlate-ſtone, and ſhells, intermixed with marine vegetables. The Doctor further ſtates, that after her return to port the fever was in no inſtance communicated, although the intercourſe between the ſick and well was conſtant and inevitable. The faculty would here be underſtood to draw a diſtinction between an imported diſeaſe and the imported cauſe of diſeaſe. While they are convinced that this fever cannot, with truth, be attributed to imported human contagion, they are equally well aſſured that the cauſe may be imported, and that the foul air in the holds of veſſels is a frequent ſource of miſchief, and may co-operate with the like cauſes on ſhore, in the production of diſeaſe. We are nevertheleſs confident, that if the air of our city was rendered as pure as it might be, by the removal of nuiſances on ſhore, that the cauſe on ſhipboard would ſcarcely be worth eſtimating as an object of terror, as it muſt ſoon be ſo widely diffuſed, in a pure ſurrounding atmoſphere, as not to produce any permanent effects.

The gradual manner in which this diſeaſe becomes epidemic is an additional proof that it is not derived from foreign ſources. The firſt effects produced by the cauſes, while they are feeble—ſuch as ſlight pains in the head, ſtomach and liver, and other mild grades of the diſeaſe—are the certain harbingers of a more malignant ſtate of fever, when the ſame cauſes acquire more force. If this diſeaſe were imported, the prominent features would develope themſelves at firſt, and theſe precurſors and more mild grades of the diſeaſe could not affect thouſands on ſhore who never had any communication with veſſels from the Weſt-Indies, or any diſeaſed body.

It has been ſuggeſted that the late fever might have been imported in the United States frigate Inſurgent. This ſuggeſtion, we are ſatisfied, is deſtitute of foundation. The Inſurgent arrived at this port on the 24th of March, with an healthy crew, who continued equally free from diſeaſe after her arrival, which precludes the poſſibility of perſonal contagion from this ſource. That the ballaſt of this ſhip was not the cauſe of the fever is evident from the following facts: The neighbourhood of the wharf at which ſhe was moored was the moſt healthy at Fell's Point, and the firſt caſe of fever occurred at the other extremity of that place. Of nearly two hundred perſons who were conſtantly employed on board, in repairing this ſhip, not one was among the firſt perſons attacked by the fever. The rumour that Mrs. Wilſon contracted her death-ſickneſs from expoſure to the ballaſt of the

Inſurgent, is equally invalid. Her illneſs is more rationally explained by her having paid a viſit, on the margin of the peſtilential cove, immediately preceding her attack: and hence moſt of the firſt caſes at Fell's Point may be traced; and hence they date their firſt indiſpoſition.

But what, in our minds, places this queſtion beyond the reach of controverſy, is the fact, that this fever is never contagious early in ſummer, and rarely ſo at any ſeaſon in a pure atmoſphere, remote from the exhalation of ſtagnant water, or putrefying animal or vegetable ſubſtances. If this fatal diſeaſe were derived from imported contagion, each ſucceeding caſe ſhould be as capable of multiplying itſelf as that whence it firſt originated on ſhipboard. Since, then, it is evident that this diſeaſe is not communicable beyond the ſphere of noxious exhalation, the advocates of imported contagion will agree with the favourers of domeſtic origin in recommending energetic meaſures of prevention on ſhore, however materially they may differ on the controverted queſtion of origin.

The faculty believe the following to have been the principal ſources of this late malignant fever.

1. The *cove*, which extends from the mouth of Jones's Falls to the interior parts of Fell's Point, the bottom of which was left bare, by the receſs of the tide, for ſome weeks immediately preceding the epidemic appearance of the fever. This was occaſioned by the prevalence of north and eaſt winds, which continued a great part of the ſummer, as may be perceived from a regiſter of the weather during that ſeaſon. Such is the ſituation of this peſtilential cove, that all the filth conveyed into it by the weſt, north-weſt, and ſouth winds, muſt remain to ſtagnate and putrefy under a ſummer's ſun. There can be no egreſs by any but a north-eaſt wind, and this cannot act, for the point is a barrier which effectually obſtructs the wind from that quarter.—From the united teſtimony of the phyſicians at Fell's Point, the diſeaſe began on the borders of this cove, and its progreſs could be traced through the ſtreets in whatever direction the winds wafted its poiſonous effluvia. Such was the peſtilential condition of this ſink of putrefaction, that the labourers employed in filling up its northern ſhore were compelled to relinquiſh their undertaking early in the ſummer. So offenſive were the effluvia emanating from this ſource of death, that it affected thoſe who had occaſion to paſs it even at a conſiderable diſtance.

2. The *docks* in general, but more eſpecially the *interſtices between the wharves*, where the water ſtagnated, and af-

forded a proper matrix for the generation of peſtilential effluvia.

3. The ſtagnant water retained in *cellars*.

4. The *gutters*. The noxious exhalations from this ſource, ſo near the doors of our houſes, are too obvious to require a comment.

5. Certain filthy *alleys* and *unpaved ſtreets*.

6. Unclean *back yards*.

7. Certain *privies*.

8. The *ponds* and *low grounds* in the city and its vicinity.

9. The *made ground*, of which the wharves and the lower parts of ſome of the ſtreets are formed; and from all ſubſtances, animal and vegetable, ſubject to putrefaction.

From theſe ſources we derive the firſt caſes of the late fever; and from theſe, fomented by the ſummer's ſun, we believe it to have become afterwards epidemic. We are more ſtrongly impelled to aſcribe our late malignant fever to theſe cauſes, from having aſcertained that it did not exiſt in the higher parts of the city, remote from exhalation, unleſs it had been carried there from the Point, or from the lower parts of the city. The following fact is of itſelf ſufficient to point out the ſources of our late awful calamity. The ſuperintendant of the ſtreets, who enumerated the nuiſances previous to the epidemic appearance of the fever, declares, that in whatever part of the city he then marked a *nuiſance*, he can now mark the ravages of death.

ARTICLE VII.

EXPERIMENTS *and* OBSERVATIONS *on* MERCURY: *In a Letter addreſſed to Profeſſor* HOSACK, *by* GEORGE LEE, *A. M.*

Pennſylvania Hoſpital, Sept. 12, 1800.

DEAR SIR,

HAVING lately finiſhed ſome experiments on mercury, which tend, in a ſmall degree, to correct a prevailing error of ſome magnitude, I cannot avail myſelf of a more favourable opportunity of tranſmitting them to public notice than through your hands. With this liberty I therefore hope you will indulge me; at the ſame time aſſuring you, that a high and grateful ſenſe of the kind treatment which I received

from you at New-York, a few weeks ſince, is not among the ſmalleſt of the conſiderations which have induced me to make this communication.

The ſubject to which theſe experiments were more particularly directed, preſents an intereſting example of the evil which may accrue from an imprudent and unphiloſophical application of a principle which, in its preſent unguarded extenſion, I hope to prove fallacious. I allude to a late doctrine taught by Drs. Girtanner and Beddoes—men as juſtly celebrated, perhaps, for the inventive genius of imagination, as for the uſeful and durable labours of a cool and deliberate underſtanding. Theſe gentlemen believe that mercury, when introduced into the body, owes its activity to oxygen, with which they ſuppoſe it is always combined. To accord with this doctrine, it was neceſſary to conceive and aſſert, that after the adminiſtration of this metal in a previous ſtate of oxydation, it was thrown out revived, in conſequence of having imparted its oxygen to the ſyſtem.*

Having ſtated the doctrine which I mean to controvert, it would be injuſtice, on my part, not to mention, in this place, that the authors of it have made no experiments to maintain their favourite poſition. At the ſame time it would be derogating from their fame not to add, that the opinion ſeems to have ſprung partly from analogical induction, but more from preconceived theory. To act otherwiſe, I ſay, would be unfriendly: for but few artiſts poſſeſs the power of raiſing a ſuperſtructure, the pillars of which have no foundation. This is what is commonly called reaſoning *a priori*; which is, for the moſt part, either accidentally right, or accidentally wrong. To ſay in which of theſe two points the reaſoning of our authors has terminated, is unneceſſary: ſuffice it to obſerve, that, in general, it is neither an arduous nor unpleaſant taſk, to reaſon *down*, by experiment, opinions which men attempt to reaſon up by *conjecture*. Hypotheſis may, by chance, caſt an ephemeral ray of reaſon on a ſubject, but it is *experiment* alone which can reflect the durable and animating lights of truth.

Candour induces me, before proceeding any farther, to acknowledge, that, upon my firſt acquaintance with this doctrine, I was much pleaſed with the ſimplicity of its ſtructure, as well as the ingenuity with which it had been erected. I was, during ſome time, an advocate for its prevalence; ſo in-

* Vide Beddoes on Conſumption, and Girtanner on Irritability.

ſenſibly was my mind carried into a conviction of its ſuppoſed truth. But who would not be captivated by, and admire the theory of the *modus operandi* of a claſs of medicines (called ſialagogues), which reduced it to ſo plain and ſo intelligible a point, as that oxygen was the univerſal principle of ſalivation? I forſook this belief, however, nearly as ſoon as I had attached myſelf to it: for, after ſome ſtudy, and not without ſatisfaction, its abſurdity ſtruck me in the moſt forcible manner. Why, it was natural to inquire, attribute the effects derived from the adminiſtration of mercury to oxygen, and not to the metal itſelf? or, why uſe the latter, if the former is alone the agent by which every thing is effected?

Some of the reaſons which cauſed me to ſuſpect the validity of the fore-mentioned doctrine, I will here adduce, nearly in the order in which they occurred.

1. Many ſubſtances ſalivate which do not contain a particle of oxygen; of courſe, a ſalivation may be induced without the preſence of oxygen. Dr. Barton, in his lectures on the Materia Medica, mentions the caſe of a child, who, in conſequence of ſwallowing a cent, was ſalivated. The coin, when diſcharged, was diminiſhed in ſize a fifth part. The copper, in this caſe, when ſwallowed, contained no oxygen; and to ſay that it became oxyded in the ſtomach, is an evaſion which, on account of its abſurdity, merits but little attention. It has been obſerved,* that the phoſphoric acid exiſts, in a naked ſtate, in the gaſtric juice, which may be capable of oxyding a metal. From the reſpect due to the gentlemen† who are ſuppoſed to have detected this acid, I am averſe to diſpute its exiſtence in the gaſtric juice. It is my duty, however, to obſerve, that if it does really exiſt in this fluid, its quantity muſt be very ſmall indeed, when the colour of a blue vegetable is not altered by it. This was proved by ſeveral experiments which I aſſiſted Dr. Glover to make upon the gaſtric juice of animals‡ which had previouſly faſted many hours. As a further proof of the non-exiſtence of this acid in the gaſtric juice, we have the high authority of Spallanzani, and his profeſſional colleague Scopoli, who, by accurate and well-conducted experiments, were unable to detect any other acid than the muriatic, which was combined with the volatile alkali.§ Spallanzani ſays, that when an acid is

* Elements of Chemiſtry and Nat. Hiſt. by Fourcroy, vol. iv. p. 21.
† Meſſrs. Macquart and Vauquelin.
‡ Cats and dogs.
§ Spallanzani's Diſſertations, vol. ii. p. 328—331.

found in a free ſtate in the ſtomach, it is not derived from the gaſtric juice, but is evolved by the aliment in digeſtion. This fact accords with the experiments made in conjunction with my friend Dr. Glover.* But if theſe be not ſufficient reaſons for diſbelieving the calcination of a metal in the ſtomach, how will the authors of the opinion to which this paper is oppoſed explain the operation of camphor and ſeneka, which, upon the authority of Dr. Barton, ſalivate?

2. It has been ſaid that ſulphur relieves a ſore mouth by combining with the oxygen which was the cauſe of the diſeaſe. But, in anſwer to this, were I to inquire, is the ſulphuric acid formed in this caſe? I ſhould receive but an unſatisfactory anſwer. Again—Dr. Barton ſays, in his Lectures, that he has ſeen, in a number of caſes, the effects of mercury upon the mouth abated in a very ſurpriſing manner by the uſe of opium. The illuſtrious Boerhaave has taken notice of the ſame fact, in the following extravagant words: "Opium is a heavenly and moſt certain remedy in quieting a too violent ſalivation."† I do not know that opium has an affinity to oxygen!

3. It is well aſcertained, that the heat of the human body is not ſufficient to deoxygenate mercury in the forms in which it is generally uſed. It is true that trituration alone will revive precipitate *per ſe*. This preparation contains but a ſmall quantity of oxygen, and all higher degrees of oxydation require a temperature many degrees above 98, to diſengage the oxyding principle from mercury.

4. Mercury may be rendered inviſible, by triturating it with rhubarb and honey, in the ſpace of two minutes. Is it to be preſumed that this metal can be oxyded in ſo ſhort a time? Yet, if adminiſtered to a patient immediately, it will ſalivate.

5. A few grains of corroſive ſublimate will ſalivate, which do not contain, perhaps, a thimble-full of oxygen. Can gentlemen tell you that this ſmall quantity of oxygen will produce a *febris mercurialis*, who believe themſelves in the oxygenation of the blood by reſpiration? If this paradox can be reconciled to common ſenſe, it is to be effected only by the ingenuity of a Girtanner or of a Beddoes.

I could multiply objections of this nature—I had almoſt ſaid—*ad libitum*. But this would be making an unpardonable intruſion on your patience. If the ſubject, however, is

* Vide his Inaugural Diſſertation on Digeſtion. 1800.

† Vide Acad. Lect. vol. vi. p. 398.

not ſufficiently examined already, the following experiments, I hope, will place it in a fairer point of view. They are calculated to prove that mercury, in two of its forms, is not oxyded; and, conſequently, that is action in *them*, at leaſt, does not depend on oxygen. It therefore muſt be preſumed, *cæteris paribus*, that oxygen is not the eſſential, nor only active principle, in the other preparations of this metallic ſubſtance.

One hundred and twenty grains of crude mercury, and an hundred and ten grains of powdered rhubarb and conſerva roſarum, mixed, were triturated together in a marble mortar until the mercury was inviſible. The thermometer of Fahrenheit was at 90 deg. At the end of this operation, which was continued at leaſt half an hour, the maſs was carefully weighed, and found to have loſt ſeven and a half grains in weight. Next day, the thermometer being at 82 deg. this experiment was repeated upon 240 grains of mercury, and 300 grains of the rhubarb and conſerve. The trituration was continued the ſame length of time, and the loſs of weight in the maſs amounted to $15\frac{1}{2}$ grains.—*N. B.* A very conſiderable degree of heat was generated in the maſs by the trituration, and the diminiſhed weight doubtleſsly aroſe from the conſequent evaporation.

Theſe two experiments tended not a little to convince me, that the belief in the oxydation of mercury was rather premature. Yet the poſſibility of evaporation and fixation of oxygen going on at the ſame time in the trituration of theſe ſubſtances, rendered the experiment ſomewhat equivocal. This difficulty was removed by another experiment.

Two hundred and forty grains of mercury, an hundred and ſixty grains of flour, and an hundred and twenty grains of conſerva roſarum, were triturated together, in a marble mortar, two hours, the thermometer ſtanding at 80 deg. The loſs experienced in weight was fifteeen grains.—By way of compariſon, 160 grains of flour, and 120 grains of conſerva roſarum (the ſame quantity of each uſed in the laſt experiment), were triturated without mercury, the thermometer being about 78 deg. The trituration was continued as long, and in a manner ſimilar to the laſt. The loſs which reſulted was thirteen grains.—The evident impracticability of repeating theſe triturations exactly in the ſame way, will eaſily account for the difference in the reſults of the two laſt experiments. This difference, however, proves, in an eſpecial manner, that the metal was not oxyded; for if it had been,

the additional weight it would have neceſſarily received would have rendered the total loſs in the firſt much leſs than that in the laſt experiment.

Theſe two laſt experiments being repeated upon a larger ſcale, with a correſpondent reſult, in addition to the preceding, ſatisfied me that the metal, in the hydrargyrous maſs, was not oxyded. Hence the efficacy of the *blue pill* does not depend upon oxygen. And that its virtue conſiſts in an extreme or minute diviſion of the mercury (ſo as to fit it for abſorption), is an inference which, in my opinion, can be drawn with ſtrict propriety from the foregoing experiments. Whether the ſame principles can be applied to mercurial ointment, will be determined by the ſucceeding experiments.

One hundred grains of crude mercury, and 150 grains of ſimple cerate (a compoſition of hog's-lard and bee's-wax), were unremittingly triturated together for the time of one hour and an half, and the loſs of weight was twenty grains. This experiment was repeated upon double the quantity of theſe ſubſtances, and the loſs was forty-three grains.* For the purpoſe of a comparative eſtimate, 300 grains of ſimple cerate were triturated an hour and an half, and the loſs of weight was forty-one grains.—*N. B.* The thermometer was about the ſame ſtandard in all theſe experiments—being generally at the 80th degree of Fahrenheit.

The reſults of theſe laſt-mentioned experiments coincided ſo happily with the reſults of the foregoing, that little heſitation exiſted in my mind, in its determination on the queſtion. But wiſhing to place the truth as much in the light as I could, I inſtituted another experiment, which, perhaps, is of as much importance as any yet related.

For the purpoſe of triturating mercurial ointment in a certain quantity of atmoſpheric air, I procured a common apothecary's bottle, with a large mouth, and cut the bottom off. Over the mouth of this bottle I tied a ſound piece of bladder, leaving it ſo looſe as to admit of a conſiderable concavity, when it was ſuffered to fall within the mouth of the bottle. In the centre of this bladder a crucial inciſion was made, and a long wooden peſtle was paſſed through, and the bladder tied firmly round it, about midway. I then faſtened a ſmall glaſs mortar to the ſhelf of a pneumatic tub, over which the water roſe two inches. The bottle thus conſtructed was proved to be air-tight, by immerſing it full of air, when no

* The trituration was continued one hour and an half.

bubbles aroſe from it.—Having put two ounces of freſh lard and ten drachms of crude mercury into the mortar, the bottle was placed over it on the ſhelf. A quantity of air was then withdrawn from it by means of a ſyphon; and the height to which the water aroſe, within the bottle, was marked on its outſide, making, at this time, the bladder preſerve a plane. The trituration was then commenced, and continued two hours, at the end of which time the water within the bottle roſe exactly to the ſame height at which it ſtood before the experiment was begun. The ointment had by this time aſſumed its uſual blue appearance. This experiment was repeated upon two ounces of mercury and two ounces of lard; and, after a trituration during two hours, the air had not diminiſhed, apparently, in the ſmalleſt degree. The inference, therefore, from theſe experiments, is, that if mercury is oxyded by this kind of trituration, it is not in conſequence of the decompoſition of atmoſpheric air. That this metal could have been oxyded by the lard, as has been ſuppoſed by ſome perſons, was improbable from two circumſtances: 1. The lard was not rancid, which was a proof of its not being ſaturated with oxygen. 2. Lard has a ſtronger affinity to oxygen than mercury; conſequently the latter cannot take oxygen from the former. This is proved by boiling lard upon an oxyd, when the metal will be revived.

I will not loſe ſight of the above experiments, before I add an important obſervation of Mr. Cruickſhanck, who ſays, that the "globules of quick-ſilver, in the beſt prepared mercurial ointment, may be diſtinctly ſeen in the microſcope."* This is a ſtrong argument againſt the oxydation of mercury.

It is not unintereſting to obſerve, that ſome of the ointment, as well as of the mercurial maſs, made in the above experiments, was adminiſtered to patients in the hoſpital, with the common effects of theſe remedies.

Before I bring this letter to a concluſion, permit me to make one or two additional remarks. The dark blue colour of the ointment has been aſſigned as a proof of the oxydation of the mercury it contains. This colour, however, is only natural. When the ointment is new, it is of a light aſh, or grey colour, and becomes blue gradually in time. This gradual change is not owing to the union of oxygen to the mercury: for 500 grains of freſh mercurial ointment were expoſed in

* Vide Anat. of Abſorb. Veſſels, p. 103.

a box covered with fine gauze, and, at the end of three months, it was found to have loſt 18 grains of its weight. It is true, that if a certain quantity of air be expoſed to mercurial ointment, it will be diminiſhed. In this caſe the oxygen is abſorbed by the lard, which thereby becomes rancid. A quantity of fixed air is emitted from the lard. This ariſes from a combination of part of the oxygen to the carbon of the lard. Hence a putrefactive proceſs goes on in the lard, and the metal remains unchanged. Another proof of this was afforded me, by expoſing equal quantities of mercurial ointment and lard, ſeparately, in equal quantities of atmoſpheric air, and the abſorption in both was exactly the ſame.

Gentlemen who have adopted the theory I have endeavoured to prove erroneous, have been ſo much infatuated by the perverting poiſon of enthuſiaſm, that nothing has been able to cool their ardour, or to put a ſtop to their wild career. Oxygen has been the burden of their ſong; and, in the purſuit of this queſtion, common ſenſe has been offended, whilſt prudence has been unable to mark out a ſingle limit to their range. Candour has long ſince retired from the ſcene of action: for while the heart beat ſenſibly for the truth on one ſide of the queſtion, ſelf-love and miſguided fancy proclaimed victory on the other.

It has been the cuſtom, of late, to eſtimate the powers of medicines according to the quantity of oxygen they contain. Hence the ſuperior powers of the oxygenated muriate of mercury, over thoſe of the mild muriate, have been explained in this way. May not this doctrine be applied, with equal advantage, to ſaccharum ſaturni and white lead? But to be ſerious—is not corroſive ſublimate of mercury a more complete ſalt than calomel, and on this ſtate alone do not its ſuperior powers depend? The nitric acid is more powerful than the nitrous; and it is ſaid, becauſe it contains more oxygen. Admit the fact: but, then, is it not combined with the azote? Is not the azote completely ſaturated in the nitric, and not in the nitrous acid; and does not this circumſtance conſtitute in the one a more complete acid than in the other? Azote is as neceſſary to the formation of the acid of ſepton as oxygen; and if certain effects are derived from the uſe of this acid, they are to be attributed to both its conſtituent principles, and not to one alone. When azote and oxygen are combined, they produce certain effects upon the body. Theſe are properly ſaid to be the effects of the nitrous acid. When

theſe two principles are not combined, they are either inactive, or their effects are no way analogous to thoſe of nitrous acid. The ſame remarks may be applicable to the oxygenated and common marine acids.

But after all that has been ſaid on this curious ſubject, have our authors ever proved the decompoſition of theſe different ſubſtances in the body? This is a point of ſome conſequence to them. Their future reputation reſts upon it in a great meaſure. In my opinion, an inveſtigation of the ſubject, on this ground, will either extricate them from their preſent difficulties, or advance them a few ſteps further into the mire. It behoves them, at leaſt, to make the experiment.

ARTICLE VIII.

An ACCOUNT *of* REMARKABLE EFFECTS *from the* APPLICATION *of the* ACTUAL CAUTERY *to the* NAPE *of the* NECK *and the* TOP *of the* HEAD, *in ſeveral Diſeaſes, both Idiopathic and Symptomatic, of the Eyes, Ears and Brain. By* LOUIS VALENTIN, *M. D. formerly Profeſſor of Anatomy at Cape-François, &c. &c. now of Nancy, in Lorraine. Communicated to Dr.* MITCHILL.

SECTION I.

IT is well known how much the actual cautery was uſed by the ancients; and, at this day, great is the extent of the practice among the Egyptians, the Japaneſe and Chineſe, either by the *moxa* or the hot iron, for pains of almoſt all parts of the body. It was ſo familiar with the Grecians and Scythians, that they looked upon diſeaſes as incurable when fire had not conquered them. Thereupon Hippocrates ſaid: "*Quod remedium non ſanat ferrum ſanat, quod ferum non ſanat ignis ſanat, quod ignis non ſanat inſanabile dici oportet.*" This excellent method has been renewed and celebrated in our age, by Pouteau, of Lyons.* I do not mean to relate all caſes where I have ſucceſsfully employed this help

* See the poſthumous works of that ingenious and ſkilful ſurgeon, by Dr. Colombier, of Paris. Proſper Alpin. Med. des Egyptiens. Du Jardin, Hiſtoire de la Chirurgie, tom. i. à Paris.

in the healing art, but ſhall confine my remarks merely to what concerns the principal diſeaſes of the head and face, as it has been extenſively uſed in them.

Section II.

In many kinds of obſtinate opthalmias, in the blindneſs that follows them, and in ſerious old ſores of the eyes, whatever the cauſe might be, in which all means, both internal and external, bliſters and ſetons on the hinder part of the neck, had been employed, I have always obſerved the efficacy of fire upon the top of the head to deſerve a preference. It muſt be applied about the middle of the hinder edge of the coronal bone (the place being ſhaved), and the extent of the burning at leaſt as large as a dollar. The application produces, indeed, an acute pain, but the patients are greatly relieved by the uſeful effects they derive from it. The part muſt be immediately anointed with oil, and covered with a ſoft and ſuppurative plaſter. The next day two or three crucial inciſions muſt be made into the burned ſpot, in order to accelerate the ſuppuration, which becoming more copious, and being nearer the ſeat of the diſorders than when theſe emunctories are put on the nape or occipital region, they produce a ſpeedier relief; beſides the primitive effects of the exuſtion, in changing the nervous irradiations, and attracting, as about a centre, the humours of the head. I prefer it to the other external irritants, as it maintains for a longer time a copious diſcharge of matter.

At Cape-François, where ſore eyes are common and very obſtinate, and their cure very difficult, I have cured many people by this ſalutary method, though they had already taken advice from all parts, and tried, unſucceſsfully, various kinds of remedies, for a long time; and this even when the blindneſs was acknowledged and certified by profeſſional men.*

I oftentimes ſcarified and took off portions of the tunica conjunctiva, when the veſſels were conſiderably choaked up, particularly in the *opthalmia chemoſis*, *opthalmia ulceroſa*,

* A fact the moſt known and deſperate concerns a young lieutenant of volunteers, whoſe cure the general aſſembly of the colony had truſted to me, about the beginning of the year 1792, after the exhibition of a certificate delivered to him by the two firſt health-officers of the charity-hoſpital, where he had before been treated, atteſting the loſs of his ſight, and his being paſſed as incurable. The proof of the radical cure is mentioned in the verbal procés of that aſſembly, June 24, 1792. The patient recovered his ſight in leſs than five weeks.

and many others. In all cases one or two pukes must be administered previous to the cauterization, and they should be used rather than purges.* The softening and lenient topicks and collyria ought to be preferred to those which are acrid and irritating, in the increase as well as in the decrease of the disease.

Section III.

In sore ears, as the *otalgia inflammatoria*, accompanied with delirium, convulsions or drowsiness, when liniments, topicks, cupping-glasses, leeches, or blisters, have been unsuccessfully employed, the hot iron behind the ear, upon the *processus mastoideus*, succeeds perfectly well. I have also cauterized, advantageously, the inward portion of the ear with a smaller red iron, slipping through a sheath, with a design of appeasing tooth-ache, and pains of the jaws and the contiguous parts, produced by a defluxion to that region, as some of our predecessors have done.

Section IV.

I have seen it likewise produce good effects in acute and obstinate pains of the head—that is to say, in *cephalalgia* and *cephalæa*, as well as in madness, after an unsuccessful use of other means. Then it is better to cauterize the vertex strongly, and, if it be possible, on the painful place, particularly when the fits return periodically, and have not been sufficiently overcome by the red-powder-bark.

Some have also employed a large blister-plaster, covering all the head; and in malignant fevers and commotions of the brain its effects appear very salutary.† Others have cauterized the head to the very skull, or even to the bone itself, after having cut away the hairy scalp, in madness and in epilepsy.‡ But these experiments were not always prosperous.

Section V.

It is in phrenitis, in the frightful symptoms of putrid and malignant fevers, and others of an eruptive kind, or coma,

* In general, I have observed, in all cases of sore eyes, that pukes succeed better than purges. This preference given to pukes, in diseases seated above the diaphragm, agrees with the aphorism of Hippocrates: *Quæ, supra septum transversum vomitus, et infra purgationes requirunt.*

† The blister-plaster, in the form of a cap, is known to be useful in commotions of the brain; and M. Default, chief surgeon of the Hotel Dieu, at Paris, has used it frequently, and with success.

‡ Vide Dehaën, ratio medendi.

delirium, convulsive motions, *prostratio virium*, &c. that the application of the actual cautery on the occiput is very useful. I could give an account of many remarks made in France, in the West-Indies, and particularly in Virginia, with regard to the hot iron in the treatment of the above diseases. I shall confine myself, however, to a few facts.

Mademoiselle Pellier, a superannuated maid, at Nancy, in Lorraine, living near the Botanical Garden, was attacked by an excessive phrenitis, in the year 1787, after the operation of a violent purge she had taken for the head and belly-ache. She was furious, and they were obliged to tie her in bed. Her pulse was low and frequent, and her eyes animated. She constantly refused swallowing. Repeated employment of the semicupium, and blisters put on the legs, dry frictions, cooling topicks upon the head, were inefficacious, and nothing could appease or relieve her. Having been consulted on her dangerous case, I cauterized the nape of the neck without delay. Her great agitation, flow of words, and cries, ceased immediately. She remained with her eyes open, and speechless, during two hours; after which she slept till the next morning. Her fever was moderate, her pulse stronger and more free, and that disease which had a putrid character was radically cured in a fortnight.

Section VI.

At St. Domingo, where malignant fevers of a bad character, agues and intermitting fevers, are exceedingly common amongst the white people, the cauterization of the nape of the neck, precisely between the occipital bone and the first cervical vertebra, near the superior insertions of the trapezius muscle, has very often succeeded with me, when the sick had fallen into a coma, with convulsive motions, and a closing of the jaws. Sometimes the power of swallowing was lost, besides shortness of breath, &c. If the patient is depressed, low, and sinking under the disease, notwithstanding blisters and the use of quinquina, so advantageous in this state; if the nervous system was a little affected, the brain confused, and the mind roving, and those means were inadequate to put a stop to these disorders—I apply the burning iron without delay. I made use of it early, according to the emergency, but never before having obtained the necessary evacuations; it would otherwise be useless. It may be employed at any other times, towards the sixteenth, seventeenth, or twenty-first day; seldom later in those climates.

Two young men in Cape François underwent this operation at an advanced period of their ſickneſs, after which the criſis* and cure appeared to take place. The one named Vetcon (chevalier) fell ſuddenly ſenſeleſs on the ſeventeenth day: the teeth cloſed, the belly ſwelled, and he was rapidly declining under his ſymptoms, though he had all along received regular treatment and attendance. He at laſt threw up all his medicines.—The other, named Arnould, a watch-maker,† was ſtruck with the ſame deſperate ſymptoms on the ſixteenth day; having had, till that time, a ſimple remittent fever, regularly treated. Convulſions and dreadful howlings, affrighting the neighbourhood and the people paſſing by, not being abated by cold water poured on the head, bliſters on the legs, aſſiſted by ſinapiſms, &c. I fully cauterized the occiput. He ſoon began to ſwallow ſome ſpoonfuls of an anti-ſpaſmodic mixture and powder-bark; and the diſeaſe was happily terminated, by degrees, in the former about the thirty-firſt day, and in the latter the twenty-ſecond.

Section VII.

The ſame operation was very advantageous to us in the fatal epidemic diſeaſe which afflicted the ſeamen of the French ſquadron which was at anchor in Cheſapeake-Bay, in the months of February, March and April, of the year 1794. Theſe men, miſerably diſtempered, were placed in our three hoſpitals, eſtabliſhed on both ſides of Elizabeth-River, near Norfolk.

The character of the diſorder was a putrid and malignant fever. Some were troubled with worms and peripneumony. The greater part were brought to me in a ſtate of extreme debility and deſpondency—being often in a delirium—having a blackiſh looſeneſs, little fever, and ſcarcely ſixty-eight or ſeventy-two pulſations in a minute. There were ſome in whom the vital ſtrength languiſhed ſo much, that I found only thirty-ſix, forty-four, forty-eight, and now and then from fifty to ſeventy, pulſations in a minute.

Others, very much animated, and ſtronger, having a full and frequent pulſe, became ſoon frantic, and, getting out of their beds, wandered up and down, and obliged us to tie them. For ſome of theſe I preſcribed bleeding, with anodyne and

* Thoſe diſeaſes are commonly ſhorter in the Antilles than elſewhere: in general, the criſes are never obſerved, becauſe of the early and ſo amazingly profitable uſe of the quinquina.

† From Nancy.

antiſpaſmodic remedies. Sometimes they refuſed them. But nothing abated thoſe ſymptoms better than cauterizing. They recovered ſooner and more eaſily than the former, to whom this operation was made in the ſtate of delirium and of tremours in the extremities. Bliſter-plaſters and quinquina kept almoſt always an equal pace together: ſometimes Huxham's tincture, with camphor and vitriolic acid.

Many of thoſe patients underwent the hot iron. One day, in my morning viſit, I cauterized eleven of them in a ſingle hoſpital, and we were ſatisfied that many owe their ſalvation to it. Fire has two effects: 1. In awakening and reanimating the benumbed ſenſitive principle in the former, or the weakeſt patients; in changing the nervous irradiations more ſuddenly, and having a tendency to deſtroy the other irradiation ſo hurtful to the ſyſtem, &c. in the latter. 2. By determining, in both, a more copious flow of humours from the tela celluloſa (ſince the ſkin is diſorganized), and during a longer time than can be effected by bliſters.

We ſaw ſome parotids, but almoſt all were mortal, notwithſtanding the ſtimulating topicks. The cautery applied to the tumours was more beneficial. At length, attendance failing, many died, becauſe the health-officers, as well as myſelf, inhaled the poiſonous vapours, and ſickened in conſequence thereof.

ARTICLE IX.

To the Editors of the Medical Repoſitory.

GENTLEMEN,

If the following obſervations appear to deſerve a place in your Repoſitory, the inſertion of them will oblige your moſt obedient, *F. BLANCHET.*

New-York, 24th Auguſt, 1800.

READING, in the "Philoſophical Magazine" of London, an account of the circumſtances which led to Dr. Mitchill's theory of peſtilential fluids, I find that the reaſon aſſigned for the prevention of chemical combination between the azote and oxygen in the ordinary ſtate of the atmoſphere, is, that theſe elements, united to a ſufficient quantity of calo-

ric to render them gaſeous, are repelled beyond the ſphere of chemical attraction, and, of conſequence, remain in the ſtate of mechanical diffuſion.

This explanation, though extremely ingenious, is not entirely free from objections. For if, according to the ſuppoſition of Dr. Mitchill and Citizen Guyton, atmoſpheric air can form an acid by the condenſation of the integral molecules of the two gaſes (viz. of azote and oxygen) which compoſe it, how happens it that cold, which always produces condenſation of the conſtituent particles of bodies, does not form nitric acid? Agreeably to the hypotheſis of theſe two diſtinguiſhed perſons, it ought to follow, that the ſpontaneous formation of nitric acid in the atmoſphere ſhould be more frequent in the frozen zones than in the torrid zone. But this ſeems to be contradicted by experience.

Beſides, whether our attention be extended over the globe, or confined to the narrow limits of a laboratory, it appears that the formation of nitric acid is produced by caloric, the effect of which muſt be expanſion rather than condenſation. In conſequence of this fact, which appears to be invariable, it neceſſarily reſults, that caloric, in certain circumſtances, exerts a chemical action upon theſe two bodies, and changes, by this combination, their individual nature into one new and peculiar. Further, it is reaſonable to ſuppoſe, if caloric be a ſubſtance, that it ſhould, like other ſubſtances, exert a chemical attraction for matter. It is, doubtleſs, on account of our ignorance of the laws of this attraction, that nature preſents compoſitions which ſtill aſtoniſh us, and baffle all the powers of our minds.

From the conſiderations thus briefly ſtated, I ſtill venture to differ in opinion with theſe celebrated chemiſts. But, whatever may be my predilection for my own notions, I hold myſelf always ready to ſubmit to evidence. Like thoſe gentlemen, I am in ſearch of truth. I am entirely convinced, as they are, that truth is eſſential to the happineſs of man. I ſhould be far from wiſhing to obſcure it by any miſtakes of mine. I am apprehenſive that but too often it is only to be found at the *bottom of the well.* Whatever may be the danger of deſcending, I am willing, in queſt of ſuch an object, to run the riſque of plunging into the water.

ARTICLE X.

OBSERVATIONS *on certain* OBJECTIONS *of Dr.* JOSEPH PRIESTLEY *to the* ANTIPHLOGISTIC SYSTEM *of* CHEMISTRY. *By* JAMES WOODHOUSE, *M. D. Profeſſor of Chemiſtry in the Univerſity of Pennſylvania, &c.*

No. III.

To the Editors of the Medical Repoſitory.

GENTLEMEN,

THIS letter will finiſh my reply to the objections of Dr. Prieſtley to the new theory of chemiſtry, and I ſhall not make any more obſervations on the ſubject until I hear from the Doctor.

Of the Production of the Carbonic Acid, or Fixed Air, from heating Copper, Silver and Lead, in pure Air.

An objection of Dr. Prieſtley, to the opinion that fixed air is compoſed of oxygen and carbon, is, that this gas is obtained by heating certain metals in pure air, which, according to the French chemiſts, do not contain charcoal.

To decide this queſtion, a quantity of copper was precipitated from a clear ſolution of blue vitriol in diſtilled water, by iron tacks, which were previouſly expoſed to vitriolic acid, diluted with water, in order to ſcale them. When the copper was collected, vitriolic acid and water was added to it, to diſſolve any particles of iron it might contain, and it was well waſhed in pure water.

The focus of a lens was thrown upon a portion of this metal, confined in fifty-ſix ounce meaſures of oxygenous gas, which had been waſhed in lime-water, and was of the purity of 155, until nineteen ounce meaſures were abſorbed: the remaining air was of the purity of 140, and contained $\frac{1}{100}$ parts fixed air.

The copper calcined in this operation was then revived, by heating it in forty-eight ounce meaſures of hydrogenous gas, from malleable iron, when eighteen ounce meaſures of the air diſappeared. The remainder of the inflammable air contained no fixed air.

The revived metal was then melted in fifteen ounce meaſures of pure air, of the ſtrength of 140, until eight ounce

meaſures were abſorbed. The remaining air was of the purity of 125, and contained $\frac{1}{100}$ parts fixed air. The calcined metal was again heated in forty-four ounce meaſures of hydrogenous gas, until ſixteen ounce meaſures diſappeared.—The remaining air contained no fixed air.

The revived metal was then heated in twelve ounce meaſures of pure air, until four ounce meaſures were imbibed by it. The remaining air contained $\frac{5}{1000}$ fixed air. It was then repeatedly heated, alternately in inflammable and pure air, and always with the ſame reſult. The remainder of the pure air uniformly contained ſome fixed air.

Similar experiments were made with the lead of the ſhops, and lead precipitated from its ſolution in the acetous acid by plates of zinc; with the copper of the ſhops, the copper money of Great-Britain and the United States, and copper revived by inflammable air from the calx of copper, precipitated from blue vitriol by ammoniac; and with ſilver precipitated from its ſolution in the nitric acid by ſtrips of copper. In ſeveral of the experiments no fixed air was procured by heating theſe metals in pure air: no abſorption of this gas took place when ſilver was heated in it, for the innumerable bright ſurfaces of the metal reflected the rays of light, and no impreſſion could be made upon it by the lens.

Having acknowledged the fact, that fixed air is generally obtained by heating the pureſt ſilver, copper and lead, that can be procured in pure air, the next thing is to find the charcoal, which is one of the component parts of this gas.

As the metals are obtained from their ores by means of coal, it is not unphiloſophical to ſuppoſe, that a ſmall portion of this ſubſtance is mixed with them in the operation of reduction.

When lead is precipitated from its ſolution in the acetous acid by zinc, or copper from blue vitriol by iron, a portion of theſe precipitants is diſſolved, and the coal ſeparated from the diſſolved part will be depoſited with the precipitated lead and copper.

When fixed air is generated by heating copper in pure air, which has been revived by inflammable air from the calx of copper, precipitated by ammoniac from blue vitriol, the only ſource of the coal can be from particles of duſt which accidentally become mixed with the copper, and which it is difficult to exclude.

The diſintereſted muſt decide whether this explanation is completely ſatisfactory.

SECONDLY. *Of Manganeſe heated in Inflammable Air.*

In my anſwer to Dr. Prieſtley's conſiderations on the doctrine of phlogiſton and the decompoſition of water, publiſhed in the fourth volume of the American Philoſophical Tranſactions, it is particularly mentioned, and a number of experiments are related to confirm the poſition, that when metallic oxyds are heated by a burning lens, in hydrogenous gas, that the air vaniſhes, and that its diſappearance is always in ſtrict proportion to the pure air which the calces contain. An argument which was conſidered as concluſive was brought forward to prove this fact. One drachm of manganeſe, expoſed to the action of the lens in hydrogenous gas, made exactly twenty-two ounce meaſures of the air diſappear in ten minutes; but when the pure air was previouſly driven off from it, by means of a red heat, it would make none of the air vaniſh, although it was heated in it one hour.

In reply to this, Dr. Prieſtley tells us (Med. Rep. vol. iv. p. 137), that manganeſe, out of which the pure air had been expelled by a ſtrong heat, when heated in inflammable air, produced the ſame reſult as if freſh manganeſe had been uſed, except *that much more heat was neceſſary* to make it imbibe the air.

The two experiments made by the Doctor, relating to this part of the controverſy, do not warrant the concluſion which he has drawn from them. No comparative trial was made with freſh manganeſe, and an equal quantity of the ſame kind of calx from which the air had been driven off by heat. He acknowledges that manganeſe, containing dephlogiſticated air, imbibes inflammable air *very rapidly*, and with a very *moderate degree of heat*; but he does not ſay in what manner the oxygen acts to produce this effect.

The Doctor ſuppoſes, that when the calx of manganeſe is heated in inflammable air, the oxygen it contains enters into the phlogiſticated air which is found in the reſiduum; but there is no occaſion to make this ſuppoſition, for phlogiſticated air is contained in all manganeſe, and is expelled from it by heat. When I expoſed three drachms of this calx in fifty-ſix ounce meaſures of hydrogenous gas, fifty-four ounce meaſures of the air diſappeared, and two ounce meaſures of phlogiſticated air remained behind, which was previouſly contained in the manganeſe, and was expelled from it by the heat of the lens.

Since, then, when manganeſe, and other pure metallic calces,

are heated in inflammable air, fixed air is not produced; if the pure air which they contain parts from them by the heat of the lens, and cannot be found, in an uncombined ſtate, in the air which remains behind; if the ſource of the phlogiſticated air which is ſometimes produced can be pointed out in a moſt ſatisfactory manner, what becomes of the pure air of the calces, if it does not unite to the hydrogen, and form water?

When manganeſe has been expoſed to the low red heat of a common furnace, a part only of its pure air is diſſipated from it; and the eighteen-inch lens of Dr. Prieſtley being ſuperior to the heat of the furnace, will expel more of the air, which, when the operation is performed in inflammable air, will unite with it, and make ſome of it vaniſh; but the eleven-and-a-half-inch lens which I uſe being not ſuperior to the heat of a furnace, will not expel the remainder of the pure air from the manganeſe, which has previouſly been expoſed to heat; conſequently, when it is heated in inflammable air, none of the air will diſappear.

Let Dr. Prieſtley make a fair comparative experiment, as I have repeatedly done, with freſh manganeſe, and ſome from which the pure air has been expelled by heat, and he will find that the former will make much more inflammable air diſappear than the latter.

THIRDLY. *Of Finery Cinder heated in carbonated Hydrogenous Gas.*

A probable evidence of a calx containing oxygen or dephlogiſticated air, ſays Dr. Prieſtley, is, that when it is revived in inflammable air, fixed air is produced. But this is not the caſe when finery cinder is revived in theſe circumſtances, although I purpoſely prepared ſome by melting iron in the open air.* In another place he tells us, that a very ſmall quantity of oxygen is attached to the ſcales of iron, becauſe a barely perceivable quantity of fixed air is ſometimes found, when they are revived in inflammable air.†

The obſervation that oxygen, in a calx, is, perhaps, moſt eaſily detected by its forming fixed air when it is heated in inflammable‡ air, is not true.

No fixed air is generated by heating the pure calces of red precipitate and manganeſe, which are acknowledged to con-

* Doctrine of Phlogiſton eſtabliſhed, p. 14.
† Ibid. p. 12.
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tain oxygen, in inflammable air from malleable iron; but if carbonated inflammable air be uſed, fixed air is always formed from the oxygen of the calces uniting with the coal held in ſolution in this gas.

The ſame reſult takes place from heating finery cinder in theſe airs. Water is formed when pure inflammable air is uſed, and fixed air when carbonated inflammable air is tried.

The focus of a lens was thrown upon one drachm of finery cinder, obtained from paſſing ſteam over red-hot iron-wire, confined in eight ounce meaſures of carbonated hydrogenous gas, from the flowers of zinc and coal, and which had been well waſhed in lime-water. No abſorption took place. One meaſure of the remaining air, thrown in an eudiometer tube, over lime-water, gave $\frac{10}{100}$ parts of a meaſure of fixed air.

The reaſon that Dr. Prieſtley obtains more water from heating finery cinder in pure inflammable air than from red precipitate, is, that the water, which is one of the conſtituent principles of this ſubſtance, is ſeparated from it, and another portion generated, by the oxygen uniting with the inflammable air.

ARTICLE XI.

LUMINOUS APPEARANCE *of* OCEAN-WATER *cauſed by* ANIMALS: *In a Letter from Profeſſor* MITCHILL, *of New-York, to Profeſſor* BARTON, *of Philadelphia.*

SO obvious an appearance as the phoſphoreſcence of ocean-water was aſcribed to animals as long ago as the time of Pliny. Some of theſe adhering to the rigging of veſſels in windy and ſtormy weather, and ſhining in the dark, ſeem to have cauſed the appearances known formerly by the names of Caſtor and Pollux. Much has been offered on this ſubject, both phyſically and hiſtorically, you know, by CHARLES FREDERIC ADLER, in his Diſſertation *de Noctiluca Marina*, which is well worthy of peruſal. He has given a magnified figure of a microſcopic worm, which, in the ſea of China, makes the ſalt-water luminous. It is called, by ſome French writers, *ſcolopendre marine luiſante*, and by Linnè, *nereis noctiluca*.—Some of the *ſea-jellies* and *ſea-blubbers*

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have alſo been long known to be occaſionally luminous. The principal part of theſe are "meduſas," which, at a very early day, obtruded themſelves on the attention of naturaliſts.—But, notwithſtanding all this, the philoſophical world ſeems ſtill to be a good deal undecided as to the real cauſe of the phoſphoreſcence of the ocean. I have therefore recorded the following facts, which fell under my own eye, hoping they may not be without their uſe in explaining the phenomenon. PENNANT, in his Britiſh Zoology, vol. iv. expreſſes very juſt ideas on the ſubject.

On the evening of Saturday, the 13th of September, 1800, about high-water, as we were preparing, between ſeven and eight o'clock, to bathe in the bay, my attention was called to a remarkable luminous appearance at the water's edge on the beach. The wind was from the ſouthward, and the day had been ſo warm, that about 2 P. M. the quickſilver in a thermometer hanging in a ſhaded piazza, on the ſouth ſide of the houſe, had riſen to 89 deg. At this moment it ſtood as high as 76. The diſtance from the chamber windows to high-water-mark is 210 feet. It ſeemed as if the beach was covered with coals of fire, and that bright ſparkles were conſtantly emitted among them. The ſmall undulation which moved to land looked like a wave of flame rolling along the ſhore. And the water beyond, to the diſtance of a few rods, exhibited frequent corruſcations of extraordinary brightneſs. On going down to the water, I found the ſand covered with molluſca-animals, the greater part of which were the *meduſa ſimplex*. Thouſands of them had juſt been left by the receding tide, and were yet alive. Being incapable of living long out of the water, and unable to ſurvive until the next flood, they ſeemed to be under the influence of their laſt vital movements. They lay ſo thick under foot, that at every tread many of them were cruſhed to pieces. Beſides their ſpontaneous power to become luminous, whenever they were moved they emitted light; and this happened indifferently whether their gelatinous bodies were agitated through the medium of air, water, or the direct contact of the feet or fingers. On walking among them, and thereby exciting their luminous action, the beach reſembled melted metal in a red-hot ſtate; or the phenomenon might be compared to a radiant glory ſurrounding the feet, to the diſtance of a foot and a half, or more, at every tread. In ſeveral inſtances, the light emitted by a ſingle one, when taken, in its freſh and vivid ſtate, into my hand, was ſufficient to enable me to determine the time of

night by my watch, the minute and hour hands being plainly to be ſeen: but this brightneſs was but tranſient. Frequently the creature emitted not a particle of light, and then, on a ſudden, the luminous appearance would be conſpicuous, and as quickly diſappear. The ſucceſſion of theſe lucid emanations from the creatures lying in ſuch numbers upon the wet ſand, reſembled, if ſmall things may be compared with great, the unſteady light of the fixed ſtars; and, indeed, the twinkling of theſe phoſphoreſcent animals below, and of the celeſtial bodies above, afforded a ſpectacle ſo ſingular and ſo ſplendid, that I ſpent good part of the evening in admiring it. I remarked, alſo, that the ſand on which theſe animals were left by the tide was luminous; and found, on cruſhing them to pieces in my hand, that a faint phoſphoreſcence was imparted both to the ſand and to my ſkin; but in neither caſe did it laſt long.

But what was as ſingular as any part of the phenomenon, was the effect produced by them upon our bodies and clothes while we were bathing. Whenever the water near one of them was agitated, a large fire-ball ſeemed to burſt upon the view beneath the water; and whenever this happened in contact with one of the limbs, cauſed ſome of us inſtantaneouſly almoſt to ſtart, for fear of being burned. Where any of the ſlimy matter was left, ſomewhat of a luminous appearance was perceptible on our clothes, giving them and our ſkins ſometimes the appearance of phoſphoric ſpots, and ſometimes the more extraordinary ſemblance of being painted over with liquid fire. There were more than one ſpecies of animal; for, beſides thoſe which, by their magnitude, were very plain ſubjects for examination, there were ſome luminous ſpots as ſmall as points, and as minute as the eye could diſcern. Theſe adhered to our clothes and ſkins, and, when taken up with the water in veſſels of glaſs, were too ſmall or too pellucid to be diſtinguiſhed by the naked eye, though they contributed eminently towards the effect of this ſubmarine illumination. This, I preſume, was the *nereis noctiluca*, an INTESTINAL animal. I ſucceeded in diſcovering another ſpecies, which was a ſlender worm of about a quarter of an inch long, and emitted, at times, a bright light of a greeniſh hue. This was probably a larger ſpecies of nereis.

Not being well acquainted with the nature of thoſe molluſca beings, I apprehended ſome inconvenience from ſuch a maſs of animal matter left on the ſhore to putrefy ſo near the houſe. But my cauſe of alarm vaniſhed before the next riſing

ſun: for, during the receſs of the tide, they all periſhed; though, inſtead of remaining a great maſs of dead gelatinous creatures on the beach, this whole collection of living animals, whoſe vivid exhibitions of light evinced their being alive near midnight, had ſo totally diſappeared before four o'clock, that no other trace of their having been there could be diſcerned, than ſome phoſphoreſcence in the ſand, on being ſtirred up by the foot or taken up in the hand. They had loſt their organization, and, in ſo ſhort a time, diſſolved into a kind of ſlime, which penetrated the ſand, and was mingled with the water of the next flood. On examining the ſpots at ſix o'clock next morning, before the beach was covered by the tide, not a veſtige of one of this numerous ſhoal of animals was diſcoverable by day-light. A ſpectator of the morning-ſcene could not have known, by any thing diſcernible, that ſuch a brilliant exhibition had been made there the preceding evening, or that a ſingle animal had been caſt aſhore on the ſpot.

I put ſeveral of the larger ſpecies of theſe creatures into glaſſes of their native ſalt-water, and carried them into my chamber. During the night I made repeated obſervations upon them, and found that the light they afforded was neither perpetual nor extended to their whole bodies. It was intermitting, and confined to certain lines paſſing from one extremity of the creature to another. The light was of a bluiſh colour, and the ſtreams of it were highly beautiful. Theſe animals lived all night in the water, and were as lively as ever in the morning. They were almoſt tranſparent, and nearly of the colour of the fluid in which they were ſuſpended; yet, on placing them in a good light, they were found ſufficiently opaque to be diſtinctly examined. Their figure was globoſe, or rather elliptical, ſhaped like a walnut. The largeſt were about an inch and a half in length, and the leaſt as ſmall as the eye could diſcern. Their ſtructure was too delicate to allow them to be examined in any other manner than in their floating ſtate. They were about of the ſame weight with ſea-water. At their option they could readily aſcend and deſcend in it. And as they have no air-veſſels like fiſhes, they accompliſhed their riſe and fall by a mere change of their ſpecific gravity, contracting themſelves into a ſmaller volume if they wiſhed to ſink, and expanding themſelves to a wider bulk if they intended to ſwim. Before ten o'clock next morning ſeveral of them were evidently dying, and before forty-eight hours had elapſed all of them were dead, and ſo entirely diſorganized that not a film or membrane was left; but the wa-

ter, which was a little turbid, had a ſmall mud-like ſediment, and ſmelled ſtrong of phoſphorated hydrogenous gas.

The nearneſs of their approach to pellucidity diſplayed their internal ſtructure to the eye without the trouble of anatomizing. They might be looked through without the aid of a diſſecting inſtrument; and their blood, though not *red*, but nearly of the pale colour of their bodies, reflected light enough in the day-time to enable it to be ſeen in motion, briſkly circulating through the arterious and venous tubes. This view of the circulation of the blood through the whole economy of a healthy animal was one of the moſt intereſting appearances in animated phyſiology that I ever had beheld. This creature, like the echinus, and many more, had no heart, but the veſſels were endowed with muſcular power enough to propel their fluids without the aid of ſuch an organ. The pulſations of the arteries could be eaſily counted, and the little waves of the circulating fluid diſtinguiſhed as they paſſed from the larger extremity, where the motion was moſt evident, to the ſmaller, where it was more evaneſcent, and terminated in correſponding veins. In theſe animals the circulation not only proceeds, as in other creatures, one while ſwiftly, and at another ſlowly, but, at times, is totally intermitted or ſuſpended, and this, ſeemingly, *ad arbitrium*. Eight large arteries received the pale blood from a common trunk, and conveyed it from one extremity toward the other. They were about equidiſtant, and gave the animal a ſomewhat ſtriped appearance, ſuch as a ſlight intermixture of arſenic imparts to glaſs. The termination of theſe arteries in continuous veins was very plain to be ſeen, until their ramifications upon the parts which appeared to be nutritive viſcera became too minute for ſight, after which the inviſible tubes ſeemed to connect their branches into a common canal, or vena cava, whence the eight arteries before-mentioned derived their ſupply.

Such being the manner in which their juices circulated, it could now be underſtood in what part the luminous exhibition was made. I was ſoon ſatisfied it was in the *arteries*, and not in the veins; and the diaphanous conſiſtence of the animal permitted this light to be ſeen through its ſubſtance as plain as through cryſtal. The blood, after entering the arteries, and during its ſubjection to their action, became luminous, and paſſed through like ſtreams of ignited metal or electric emanations. And this vaſcular illumination, variouſly refracted in paſſing through the pellucid ſubſtance of the crea-

ture, through the water in which it floated, and through the atmoſphere to the eye, made the whole body, when ſeen at a diſtance, appear luminous. Theſe ſtreams of light, however, were not conſtant. They not only intermitted when the circulation of the blood was intermitted, and the action of the veſſels ſtopped, but very commonly, while the fluids were paſſing through their tubes with the utmoſt rapidity, there would be no phoſphoreſcence at all. Then, again, one or more, and ſometimes all the arteries, would ſuddenly exhibit the lucid phenomenon, and the creature and the ſurrounding water flaſh with light. This luminous evolution was not confined to the nocturnal exiſtence of the animal; for, on attentively examining their functions by day, when the light was too faint to be ſeen, it could be eaſily diſtinguiſhed, by the *colour* of the fluid in the veſſels, that the ſame action was going on. It appeared of a bluiſh, or ſomewhat irideſcent hue, along the courſe of the arteries, though its feebler light, like that of a taper, was loſt amid the ſplendour of the ſolar rays. The ſame proceſs, however, is going on during the day-time, in theſe creatures, though unobſerved, that we behold in the dark.

One of theſe meduſas may be compared to a glaſs lanthorn, freely permitting the light produced within it to diffuſe itſelf to the ſurrounding ſpaces, while an external obſerver can thereby diſcern what is going on in the inſide. I am ſo entirely ſatisfied of the connection between this *evolution of light* and the *circulation of arterious blood*, that I want no clearer evidence to that point. It is probable that in an animal which is entirely deſtitute of lungs as well as of heart, the pulmonary function may be performed by the vital veſſels themſelves. In theſe creatures I believe this to be the fact.

The light, then, which theſe marine animals exhibit, may be concluded to be produced by a function in them analogous to the reſpiration of animals which are of larger ſize and more complicated ſtructure. The only reaſon why it is viſible from their bodies is, that the gelatinous matter of which they conſiſt is tranſparent. It is not improbable the ſame phenomena would be as obvious in the bodies of other creatures, and of even human beings, if the opacity of the materials of which we conſiſt did not hinder the light within us from ſhining ſo as to be ſeen.

The remarks hitherto made chiefly refer to the larger ſpecies of light-emitting creatures. The ſame apply, as far as I can judge, to all the ſmaller ſpecies. A veſſel of the water, con-

taining no *viſible* animalcula, was carried, the ſame evening, to my chamber. When agitated from without it ſparkled; and, if ſtirred by the finger within, the number of ſhining ſpots increaſed. When left to reſt, lucid points were frequently to be ſeen arranging themſelves at the ſurface of the water, where it touched the inſide of the glaſs. Some of this water, that was poured upon the table, appeared full of ſhining points, like ſparks of fire. There was not a doubt in my mind, that theſe phenomena, like the others, were animalcular; yet, on viewing the water in the morning, it was as tranſparent as ſea-water uſually is, and not a ſingle creature was to be diſcerned in it. The preſumption, therefore, is, that this form of marine light is alſo owing to animalcules, though too ſmall to be diſcerned by the eye unaſſiſted by optical glaſſes. I am ſomewhat doubtful whether the tranſparency of theſe minute light-emitting creatures would allow them to be diſtinguiſhed if I had had a microſcope with me.

Reaſoning in this manner, I became perſuaded that the luminous appearance of ocean-water in other caſes, wherein no flaſh or ſparkle was diſtinctly to be ſeen, or traced to an individual point, but an indiſtinct glow alone appeared, as around the blades of oars, or near the bows and rudders of veſſels in motion, that the phenomenon was, in like manner, referable to animalcular action. And I terminated my ſpeculation by concluding, that the light emitted from millions of theſe viewleſs aquatic animals concurred to produce the general luminous appearance of the ocean in which they float, by the ſame rule that innumerable cluſters of fixed ſtars, as modern aſtronomers teach us, produce the luminous ſpots or milky hue of thoſe portions of the heavens where Omnipotence has placed them.

I have ſaid nothing about the manner in which the light is evolved in theſe animals. If it is by the decompoſition of oxygenous air that light is ſo freely emitted, what becomes of the caloric? for their bodies are not ſenſibly heated, nor warmer than the fluid in which they ſwim. Still it is not to be denied, that in the decompoſition of that *triple* fluid there may be an evolution of *caloric* ſufficient for the economy of creatures ſo ſmall, and of a ſtructure ſo nice and exquiſite as theſe, and yet not be meaſurable by our thermometers; and that *oxygen* may mingle with their fluids in due quantity. And it may be eaſily conceived, that during the proceſs *light* may be evolved, ſufficient, in rapidity and quantity within

them, to occaſion, by tranſmiſſion through their pellucid forms, the phenomena of theſe and all other luminous oceanic meteors.

The animalcular origin of this phenomenon is corroborated by this additional conſideration. At the time when the ſalt-water is remarkably luminous in ſome places, there is very little of it to be ſeen in others, though but a few rods diſtant. At ſuch times, I found that by taking up parcels of the water, and viewing it, there were abundance of lucid points in thoſe which were moſt luminous, and but few in thoſe which were feebly ſo. The light proceeding from theſe inviſible INTESTINAS, when nearly and narrowly examined, radiates from numberleſs ſhining particles or centres; and, when viewed from a diſtance too remote to permit theſe lucid centres to be ſeen, the effect produced on the organ of viſion is a general and weaker impreſſion, as if every drop of water was luminous, by reaſon of ſomething chemically diſſolved in it.

The ſame creatures are not capable of emitting light for a long continuance of time. If ſhaken, touched or agitated, their luminous faculty ſoon becomes exhauſted, and the water in which they float immediately returns to its ordinary colour. This is ſo much the caſe, that if a very luminous portion of water (I do not mean a current) be much moved by the limbs of ſeveral perſons ſwimming or wading in it, its luminous quality will, in a few minutes, be ſenſibly diminiſhed. Therefore, when a boat is rowed along, or a ſhip is moved by the wind, or when a ſtream ruſhes over rocks, or againſt any kind of obſtacle, the light emitted in ſuch inſtances is produced by a ſucceſſion of animals, each of which, on being ſtimulated, evolves, in its turn, a certain proportion and duration of light, and in this manner gives continuance or permanency to the phenomenon.

In October, 1772, Mr. Forſter obſerved the Southern Ocean, beyond the Cape of Good-Hope, illuminated in a ſimilar manner (Voyage round the World by Cook and Forſter, vol. iii. p. 45). The curious inquirer will be ſtruck with the correſpondence of the phenomena off Table-Bay, and thoſe in Long-Iſland Sound. The circumnavigator aſcribed the luminous appearance of the ſea-water, in 34 deg. ſouth latitude, to animals of the ſame genus which cauſed it in 40 deg. 40 min. north. He ſaw the ocean luminous in 58 deg. ſouth, during March, 1773, while the weather was tempeſtuous, and ſo cold that the quickſilver in the thermometer was as

low as $33\frac{1}{4}$ deg. at noon. Thus it appears theſe creatures can live in water of any temperature above the freezing point. (Ibid. p. 97).

An opinion has been entertained, that, during the *ſhining of the ſun, its rays are abſorbed by the ocean*, and that the extrication of them again makes its water luminous. It has been conjectured, too, that the water of the ocean becomes occaſionally ſo *highly electrical*, that the brightneſs it manifeſts is but the evidence of a high charge of electricity. There has been publiſhed, too, an idea that the *phoſphoric matter* extricated from putrefying fiſh in the ocean, was the cauſe of this marine reſplendence. And others have even been inclined to think the light proceeded from ſome attrition of *ſaline particles againſt each other*, or ſome unknown combinations formed among them. The true cauſe of the phenomenon, I am inclined to conclude, is neither of theſe, but univerſally, as far as my knowledge of the ſubject extends, is a function of animals. Theſe are often conſpicuous in tempeſts, when

> High o'er the poop th' audacious ſeas aſpire,
> Uproll'd in hills of fluctuating fire. FALCONER.

And though the timid may be impreſſed with additional alarm at this appearance,

> Not ſo the man of philoſophic eye
> And inſpect ſage; the waving brightneſs he
> Curious ſurveys, inquiſitive to know
> The cauſes and materials, yet unfix'd,
> Of this appearance beautiful—— THOMSON.

It is remarkable how far theſe creatures, on ſome occaſions, are carried up our rivers. During the drought of the year 1796, when ſea-crabs were plentifully caught in the Hudſon as high as Poughkeepſie (eighty miles from New-York), the water of the river was luminous in the neighbourhood of Pollepell's Iſland, as I ſailed through the Highlands one dark night.

I forbear to trace the analogies between theſe *aquatic* animals and the numerous *aërial* ſpecies which emit light. Leaving theſe to your ſagacity and penetration, I have only to conclude by renewing the aſſurances of my reſpect.

SAMUEL L. MITCHILL.

Cedar-Grove, Oct. 1, 1800.

REVIEW.

ART. 1. *Six Chemical Eſſays, by* Joſeph Prieſtley, *LL. D. &c. Read at ſeveral Meetings of the American Philoſophical Society.* 4to. pp. 50.

THE ſheets on which theſe Eſſays are printed form the beginning of the fifth volume of the Tranſactions of the Philoſophical Society of Philadelphia. But as we poſſeſs them detached from the reſt of the volume, which is not yet publiſhed, we loſe no time in giving our readers an account of them.

That the value of the experiments related may be duly appreciated, it is proper to mention that the moſt important of them were made with a burning lens of ſixteen inches diameter, "generouſly furniſhed," ſays the author, "by Mr. Parker, who has ſo much diſtinguiſhed himſelf by his improvements in the art of grinding glaſs." To the liberality of this gentleman, in ſupplying him with various veſſels made of glaſs, Dr. Prieſtley declares the public is indebted for a great proportion of his other experiments on air.

We ſhall proceed to examine theſe pieces in the order in which they ſtand.

1. *Experiments on the Tranſmiſſion of Acids and other Liquors, in the Form of Vapour, over ſeveral Subſtances in a hot Earthen Tube.*

The reſults of ſending the vapour of ſpirit of nitre over *iron-turnings*, *copper*, *perfect charcoal*, *charcoal of bones*, *melted lead*, *tin* and *biſmuth*, are conciſely ſtated. The effects of the *vapour of marine acid* and *copper* upon each other, and what happened to it when paſſed through an earthen tube *glazed on the outſide only*, and when acting upon perfect charcoal—are alſo told. Theſe recitals will be inſtructive to thoſe who are engaged in ſimilar purſuits, or who, in the courſe of experiments, employ the ſubſtances which Dr. P. made uſe of. He relates a remarkable fact concerning *cauſtic fixed alkali*. When he ſent this *in vapour*, through an earthen tube containing iron, the firſt portion that came over was ſlightly *acid*, but not afterwards. The ſame reſult was obtained in three proceſſes. What ſort of acid this was we are not informed.

The remainder of this paper is of a miſcellaneous caſt, and has no ſtrict connection with its title: for he next recites certain experiments with *perfect charcoal* and *charcoal of copper* in nitrous acid by ſolution; with ſome account of the diſſolution of *phoſphorus* and *dry-boiled beef* in the ſame. He is of opinion that nitrous air contains oxygen, from the burning of pyrophorus and charcoal of wood in it. Though he ſeems again inclined to think, if oxygen was really one of its ingredients, it ought not to retain its aërial form. We believe the generally-received idea is correct, that it is an oxygenated baſis, whether it exiſts in a liquid or a gaſeous form. If "the juice of turnſole does not change its colour by ſaturation with nitrous air," all that we infer is, that *ſuch* nitrous air is an oxyd, and not an acid; but give it oxygen enough, and it will redden litmus inſtantly. The experimenter, who works much in his laboratory, will find it uſeful to examine theſe particulars.

After theſe follow eleven articles of detached chemical intelligence. Every one of them has its value in its preſent inſulated ſtate, and will acquire additional worth when its connection with other facts is better underſtood. We are glad to ſee morſels of ſcience thus preſerved. It was directed by a great authority, "to gather up the fragments that remained, that nothing be loſt."—The experiment with Papin's digeſter ſhows, "that a ſolution of cauſtic alkali, aided by heat, in that inſtrument, made a *liquor ſilicum* with pounded flint glaſs." The ſolution of ſilex in the boiling fountain of Geyſar is *now* no myſtery.—We relate the following facts at large, as they are not only curious in themſelves, but may explain another cauſe of *ſpontaneous* combuſtion beſides thoſe commonly known. "Pyrophorus is generally made with the charcoal of vegetable or animal ſubſtances mixed with alum, or any thing that contains the acid of vitriol, and the heat by which it takes fire is occaſioned by the eager attraction of this acid for water. I accidentally found that a pyrophorus may be made of *ſulphur and iron.* Having kept a cupfull of this mixture, made up with water, in a quantity of common air, about two months, I then took it out, and left it in the cup. The next day, perceiving the cup to be warm, I emptied it upon a board, when it grew hot, ſmoked very much, gave out a ſtrong ſmell of vitriolic acid air, and, at length, became red-hot. Putting a part of it into another cup, confined by common air, the air was rapidly diminiſhed."

2. *Experiments relating to the Change of Place in different Kinds of Air, through ſeveral interpoſing Subſtances.*

In this paper Dr. P. relates ſome extraordinary facts relative to the *ſeeming* permeability of the materials of chemical apparatus by aërial fluids. That opaque veſſels of metal and earth were penetrable by heat, and that tranſparent ones of glaſs would admit both light and heat, every operator has long known. It has not ſo generally been underſtood that an earthen veſſel of inflammable air, inverted in a large jar of oxygenous air, will, on being heated by a lens, part with its contents, and, in lieu thereof, be filled with oxygenous air. Nor has the converſe of this been commonly known, that a ſimilar veſſel of oxygenous gas, inverted in a larger jar of inflammable air, will, after being expoſed to the heat of a lens for half an hour, exchange its original charge for an equal quantity of the ſurrounding inflammable air, with which it will be found wholly filled. Yet Dr. P. affirms that ſuch are the reſults of his experiments. He found the mixture of airs in the external or *glaſs* jar was a chemical one; while that in the internal or *earthen* one had undergone no change, but was of the very ſame quality with that in the glaſs jar. Of the reaſon of this difference he can form no probable conjecture.—He formed ſomewhat of a like tranſmiſſion of the two airs both ways, when heat was applied through a *common* and a *tanned* bladder, though without chemical union. The like change of place he has found to happen in various other circumſtances, and unexpected products to be obtained when nitrous air and marine acid air were employed. The diverſified trials which he made have led this *man of long practice in experiments* to conſider them among the moſt extraordinary that have occurred to him in Phyſics, and ſuch as experimenters ought to be acquainted with, in order to prevent miſtakes of conſiderable conſequence. Through ignorance of theſe facts, it is probable that hundreds of miſtakes have been made already, and that the philoſophical world is at this moment miſled by their deceptions.

3. *Experiments relating to the Abſorption of Air by Water.*

The matter contained in this paper is likely to ſhake our faith in the correctneſs of eudiometrical experiments with *nitrous air over water*. The unfitneſs of the eudiometer to indicate the purity of atmoſpherical air has been mentioned in our vol. iii. p. 212. The reaſons there given were two;

1. The variableneſs of the ſtrength of the nitrous gas; and, 2. Its attraction for water. Dr. P. has found that the whole materials of an eudiometrical mixture will combine with water; and, indeed, "if ſufficient time be allowed, all kinds of air, without diſtinction, will be wholly abſorbed by the water with which they are confined." A mixture of *nitrous* and *common air* was completely abſorbed by water in about eleven months. Another mixture of equal quantities of the like ingredients was made on October 5th, and was quite abſorbed on the 24th of next July. And a third, made on December 11th, had diſappeared on July 1ſt. A mixture of equal quantities of *common* and *inflammable airs*, fired together, was totally abſorbed between December 13th and July 19th. And that the *water* imbibed the gaſes appeared indubitable from contraſting the experiment with quickſilver; for when nitrous and common air, in mixture, were confined over that metal from October to April, there was no change diſcernible.

Independent of nitrous air, the whole of every kind of air confined with water, will, ſooner or later, be incorporated with it. But when atmoſpheric air is the ſubject of this experiment, the oxygenous part is always attracted firſt, and after that is imbibed, the remaining air, to the amount of about three-fourths, was quite phlogiſticated. But this was afterwards gradually blended with the water too. He found dilatation by an air-pump to prevent abſorption, and compreſſion by a condenſing machine rather to promote it. This piece alſo contains ſome curious facts on the change of the qualities of different kinds of air by *agitation in water*. Of theſe, the deſtruction of the inflammability of hydrogenous air is one of the moſt memorable.

4. *Miſcellaneous Experiments relating to the Doctrine of Phlogiſton.*

This piece is a recital of experiments tending to the ſame point with thoſe which have appeared in the former numbers of our work. It contains experiments on plumbago, on the revival of quickſilver in inflammable air from the precipitate *per ſe*, on pyrophorus, on the calcination of metals in common air over lime-water, on finery-cinder, on aërated *terra ponderoſa*, and ſome other ſubſtances. As this is a part of the great phlogiſtic controverſy, the whole eſſay ought to be carefully peruſed in connection with the other documents on

the ſubject. The concluding paragraph, however, is ſo remarkable that we ſhall inſert it entire.

"I have obſerved, that when a mixture of dephlogiſticated and inflammable air is exploded, *acid* is produced if there be any exceſs of the dephlogiſticated air, but *only water with phlogiſticated air*, if there be any exceſs of the inflammable air. Theſe proportions I endeavoured to aſcertain, and I found that acid is formed when 100 meaſures of inflammable air are united to 54 meaſures of dephlogiſticated air; but that only water was produced when 100 meaſures of inflammable air were united to 47 meaſures of dephlogiſticated air."

5. *Experiments on the Production of Air by the freezing of Water.*

This is the article which was publiſhed in our preſent volume, page 17. It is republiſhed here, with the following words of adddition at the end of the concluding paragraph: "Alſo, if any ſpecies of air be entitled to the appellation of *hydrogen*, it is phlogiſton, and not inflammable air." That is, we ſuppoſe, the *baſis* of inflammable air, and not the gas itſelf, is *hydrogen*, *i. e.* the *phlogiſtic radical* of water.

6. *Experiments on Air expoſed to Heat in Metallic Tubes.*

Dr. P. finds that in *metallic* as well as *earthen* tubes a mixture of dephlogiſticated and inflammable air will not explode even in a red heat, though this always takes place in tubes of *glaſs*, where there is no metallic ingredient. This he verified in tubes of *iron*, *copper*, *ſilver* and *gold*, and of *flint-glaſs*, which contains a portion of *lead*.

He thinks, in ſome caſes, *airs* are tranſmitted through the *ſubſtance of metallic* tubes, in a manner analogous to their paſſage through *bladders* and *earthen* tubes. Theſe are, indeed, exceedingly curious matters, and eminently deſerving the attention of all who purſue chemical inquiries. They diſcloſe the ſources of ſo much fallacy in artificial philoſophy, that of a large proportion of ſuch experiments, though made by able hands, and with the beſt apparatus, we ſcarcely know what to believe or what to diſbelieve. Nor is this ſcepticiſm unfriendly to truth and reaſon: for latterly we have obſerved that the exceſſive deſire—the rage we may call it—of making experiments in rooms and work-ſhops, has conſiderably diminiſhed the number of the true obſervers of nature. While engaged in minute operations with the imperfect inſtruments of human contrivance, within narrow and ſecluded apart-

ments, the genius of our time too often ſuffers the great works carried on by the perfect machinery of the Deity, through the wide range of creation, to paſs unnoticed or neglected. His laboratory is his world, and not unfrequently he knows little or nothing of what is going on out of doors! NATURE IS THE GREAT EXPERIMENTER; and what we moſt want are OBSERVERS AND INTERPRETERS OF HER PROCESSES. But man is vain and impatient, and will be buſy, though in vain. Dr. P.'s communications will have a ſtrong tendency to make his readers modeſt and diffident, and to turn their minds to becoming objects.

Towards the cloſe of this paper are ſome experiments relating to phlogiſticated air. The baſe of this, like all other kinds of air, he believes to be water; and, beſide it, to conſiſt of nothing but ſome modification of phlogiſton. He thinks, however, there is a peculiar difficulty in aſcertaining its conſtitution, as ſome of his experiments ſeem to ſhow that it contains the principle of acidity, and others that it is entirely free from it: nor do we find the difficulty removed by the experiments related.

ART. II. *A Review of the Improvements, Progreſs, and State of Medicine in the* 18*th Century. Read on the firſt Day of the* 19*th Century, before the Medical Society of South-Carolina, in purſuance of their Vote, and publiſhed at their Requeſt. By* David Ramſay, *M. D.* 8vo. pp. 47. *Charleſton.* Young.

AMERICA already ranks the author of the "Review" before us among her beſt hiſtorians. The opportunities afforded him, while he was a member of Congreſs, to acquire a correct knowledge of public affairs, and to inſpect original documents in their proper offices, were induſtriouſly improved. And the publication of the "Hiſtory of the American Revolution," and of the "Hiſtory of the Revolution of South-Carolina," was not only reputable to the literary character of the author's native State and country, but was replete with information concerning the cauſes, the progreſs and termination, of that ſtruggle which ſevered the Atlantic Colonies from the Britiſh Empire.

The compoſitions from Dr. Ramſay's pen, which have been inſerted in our work, ſhow him to no leſs advantage in his profeſſional capacity. Such of our readers as have peruſed

the addreſs to the Medical Society, inſerted in page 98 of the preſent volume; and the account of the yellow fever at Charleſton, in page 217, will already have formed a favourable opinion of the good ſenſe and diſcernment of the author. In the piece now before us he riſes in our eſtimation, while he takes a wider range of obſervation, and ſurveys the progreſs of medicine, from one improvement to another, during the laſt hundred years.

We do not mean to ſay that it contains a complete and circumſtantial account of every thing done, during the century which has juſt elapſed, to advance the intereſts of phyſic in all parts of the world; but there is ſo much information contained in it, on the different branches of the profeſſion, and ſtated in ſo orderly and conciſe a manner, that the peruſal has afforded more than a common ſhare of pleaſure. There is a polite and gentlemanly air which pervades the whole performance; and whether Dr. R. makes mention of the dead, or offers any ſentiments concerning his cotemporaries now alive, liberality and candour diſtinguiſh his expreſſions. Not a word of invidious diſcontent, nor of jealous rivalſhip, nor of angry diſputation, is to be found in this chaſte and manly pamphlet.

After ſome preliminary remarks on the ſtate of logic, of literature and ſcience, at the cloſe of the ſeventeenth century, the author turns his attention more particularly to medicine. The improvements in *anatomy* firſt engage his notice. The anatomizing of human bodies, the injection of their veſſels with various coloured fluids, and with wax; the corroſion of them by acids; the improvements in delineation and engraving; and the making of models in imitation of natural forms, are ſucceſſively mentioned. Then follow the diſcoveries in *ſurgery*; of which the modern mode of operating for the ſtone, hydrocele, aneuriſm, fiſtula lacrymalis, bubonocele and cataract, and a number of other advantages of ſurgery as practiſed at this time, are enumerated. He next treats of the *obſtetric art*, and delivers a ſketch of its hiſtory from the printing of Thomas Reynold's book, entitled "The Byrthe of Mankind," in 1540, down to the recent publications of Hamilton and Denman. The *theory and practice of phyſic* are then conſidered. After which follow BRAIDWOOD's and SICARD's *plans of inſtructing the deaf, and teaching them to ſpeak and write; the diſcoveries made by means of morbid anatomy; experiments with poiſons, and other ſubſtances, on living animals; and the eſtabliſhment of humane ſocieties*

for the recovery of perſons drowned, and otherwiſe apparently dead.

Dr. R. afterwards calls the attention of his hearers to the *cool regimen in fevers and ſmall-pox*; to the eſtabliſhment of learned ſocieties; to the erection and endowment of hoſpitals and infirmaries; and to the improvement and ſimplification of the materia medica. The cultivation of botany, and the late and great diſcoveries in chemiſtry, are the next ſubjects of the intelligent author's obſervations. The triumphs of phyſic over the ſmall-pox by inoculation, and over ſcurvy by vegetable diet and oxygen, are ſpoken of in terms at once exulting and philanthropic.

After ſome obſervations on the abatement of plague, peſtilential fevers, putrid ſcurvies and dyſenteries, from the improvements made in diet, cleanlineſs and living, during the eighteenth century, Dr. Ramſay enters a little into the *medical* hiſtory of America. From this (p. 35) we make an extract in the author's own words, that our readers may form ſome judgment of the ſubject, and his manner of treating it.

"The Rev. Dr. Cotton Mather recommended, and Dr. Boylſton (one of the anceſtors of Preſident Adams) practiſed inoculation in Boſton, as early as the year 1720. Dr. Mather received his firſt impreſſion in favour of this practice from a volume of the Tranſactions of the Royal Society of London, which gave an account of inoculation at Conſtantinople. This he ſhowed to Dr. Douglaſs, who treated the propoſal with ridicule. He was more ſucceſsful in his application to Dr. Boylſton, who began to inoculate at the riſk not only of his practice, but even of his life. The friends and enemies of the practice filled the newſpapers of the day with arguments for and againſt it. The whole of the influence which the learning and piety of the New-England clergy have always given them, was exerted; and the whole of it was neceſſary to induce their hearers to conſent to inoculation. The pactice, by degrees, extended through New-England, to New-York and Philadelphia, and, finally, to Charleſton, where it was partially adopted in 1738, and afterwards became general in 1760.

"The earlieſt publication in America, on a medical ſubject, which has come in my way, was a treatiſe on the iliac paſſion, by the late Dr. Cadwallader, of Philadelphia, printed about ſixty years ago. In this the enlightened author very ably explodes the then common practice of giving quickſilver and draſtic purges, and recommends in their place mild ca-

thartics, with the occaſional uſe of opiates. About the ſame time, Dr. Tennant, of Virginia, wrote a treatiſe on the pleuriſy, in which he brought into public view the virtues of ſeneka ſnake-root, which were before unknown. Dr. Colden, of New-York, and Dr. Ogden, of Long-Iſland, at this early period, favoured the public with their obſervations on a ſpecies of ſore-throat which was then prevalent and mortal. I have not been ſo happy as to meet with either of theſe performances, but have heard a favourable account of each of them. There are, doubtleſs, ſeveral other early writers in America, but they have not come to my knowledge.

"At the commencement of our revolutionary war, Dr. John Jones, of New-York, one of the moſt eminent phyſicians and ſurgeons that ever graced this country, publiſhed a work, entitled 'Plain Remarks on Wounds and Fractures,' addreſſed to the ſtudents and young practitioners in America. This was particularly deſigned for the benefit of the ſurgeons in the army and navy of the United States, and will long remain a monument both of the profeſſional ſkill and patriotiſm of its worthy author.

"The yellow fever, which, for eight years paſt, has infeſted our ſea-port towns, has given birth to much medical diſcuſſion; but as the writers on this ſubject are ſtill living, any obſervations on them, at preſent, would be premature.

"Fain would I point out to your recollection the many valuable and important writings of Dr. Ruſh, who has reſcued America from the reproach of producing no original writers in medicine; but he lives, and long may he live the pride and boaſt of his country; and delay, to a far diſtant day, that celebration of his fame which is only decent after the death of its ſubject. For the ſame reaſon, I muſt, in ſilence, paſs over the literary productions of Dr. Mitchill, of New-York. If we may judge of the full harveſt by the firſt fruits that have already appeared in his early youth, we may confidently indulge the hope, that, in the progress of his valuable life, he will largely contribute to the growing fame of his country.

"The firſt medical lectures in America were given, in or about the year 1763, by Dr. Shippen, the preſent Profeſſor of Anatomy and Surgery in the Univerſity of Pennſylvania; who had been prepared for the able diſcharge of this duty by the celebrated Dr. William Hunter, of London. The truſtees of the college of Philadelphia, with the venerable Franklin at their head, ſoon after enriched their inſtitution with the lectures of the enlightened Morgan, and of the judicious Kuhn,

who had been a pupil of Linnæus. One was ſtill wanting to complete a ſyſtem of medical education. The truſtees turned their eyes to young Ruſh, who had then ſcarcely completed his apprenticeſhip; and they encouraged him to proſecute his ſtudies in Europe, with the promiſe of a Profeſſor's chair on his return. With how much judgment and advantage this was done, the noble ardour for medical improvements which he poſſeſſes, and with which he has the happy art of inſpiring his numerous pupils, affords the moſt ample teſtimony. Whilſt theſe young Profeſſors were engaged in teaching anatomy, ſurgery, materia medica and chemiſtry, the venerable Bond, then an old practitioner, was appointed to illuſtrate the practice of phyſic, by chemical lectures on the caſes of patients in the Pennſylvania Hoſpital. Some of theſe original Profeſſors have been ſucceeded by younger men, who are not inferior to their predeceſſors; and promiſe to continue, in the nineteenth century, that generous ambition for medical diſtinction which had began among Americans in the eighteenth. In ſuch high eſtimation are they held, that their lectures are attended, every ſeaſon, by more than an hundred medical ſtudents.

"Much praiſe is due to the truſtees of the college of Philadelphia, who, at this early day, eſtabliſhed the firſt medical inſtitution in America, and began to confer degrees, while their country was a colony of Great-Britain. With pleaſure I add, that ſince we have been free and independent States, other medical eſtabliſhments have been made in New-York, Cambridge Univerſity in Maſſachuſetts, and Dartmouth College in New-Hampſhire: in each of which are Profeſſors of merit and talents.

"It reflects no ſmall honour on our young country, which only began to be ſettled by civilized people in the ſeventeenth century, that, at the cloſe of the eighteenth, there are in it ſo many medical ſchools; and, in particular, that in the three adjoining States of New-York, New-Jerſey and Pennſylvania, within the diſtance of 120 miles, there are three Profeſſors of Chemiſtry—Mitchill, Maclean and Woodhouſe; who, for an accurate knowledge of their department, can bear a compariſon with the Profeſſors of the ſame in the oldeſt ſeminaries of Europe. The greateſt chemiſt of the world has alſo fixed his reſidence among us, and, in the woods of Northumberland, continues thoſe philoſophical experiments which have done ſo much honour to the eighteenth century.

"Our country is now favoured with a periodical publica-

tion, under the title of the 'Medical Repoſitory,' which, in addition to many valuable original eſſays, chiefly from the ingenious editors, Drs. Mitchill and Miller, brings to the view of the American ſtudent all that is new, ingenious or uſeful in foreign publications, on the ſubject of medicine, or the arts connected therewith.

"In the medical hiſtory of South-Carolina, the yellow fever and ſmall-pox have been the moſt injurious epidemics. My information relative to theſe two diſeaſes, prior to the year 1763, has been chiefly furniſhed by Dr. Prioleau, as collected from the manuſcripts of his accurate and obſerving grand-father, the venerable Samuel Prioleau, Eſq. who died in the year 1792, at the age of ſeventy-four. From theſe it appears, that in the year 1699 a diſeaſe prevailed in Charleſton, which ſwept off a great part of the inhabitants, and ſome whole families. This was then called the plague, though afterwards ſuppoſed to have been the yellow fever.

"In the year 1732, the yellow fever began to rage in May, and continued till September or October. In the height of the diſorder there were from eight to twelve whites buried in a day, beſides people of colour. The ringing of the bells was forbidden, and little or no buſineſs was done. In the year 1739 the yellow fever raged nearly as violently as in the year 1732: it was obſerved to fall moſt ſeverely on Europeans. In 1745 and 1749 it returned, but with leſs violence: however, many young people, moſtly Europeans, died of it. It appeared again, in a few caſes, in 1753 and 1755, but did not ſpread. In all theſe viſitations it was generally ſuppoſed that the yellow fever was imported; and it was remarked that it never ſpread in the country, though often carried there by infected perſons, who died out of Charleſton, after having caught the diſeaſe in it.

"For forty-two years, after 1749, there was no epidemic attack of this diſeaſe, though there were occaſionally, in different ſummers, a few ſporadic caſes of it. In the year 1792 a new æra of the yellow fever commenced. It raged in this city in that year, and alſo in 1794, 1795, 1796, 1797, 1799, and 1800. In thoſe laſt ſeven viſitations of this diſeaſe, it extended from July to November, but was moſt riſe in Auguſt and September. With a very few exceptions (chiefly children), it excluſively fell on ſtrangers to the air of Charleſton, and was in no inſtance contagious. In the years 1796 and 1797 it raged with its greateſt violence. In the two laſt years it has conſiderably abated. In the year 1799, the whole

number of deaths from it was 239; and in 1800, no more than 134: but this decreaſed mortality was partly owing to the decreaſed number of ſtrangers; for ſuch were cautious of viſiting Charleſton in the warm months. In Philadelphia it began with great violence in the year 1793; and from that time to the preſent it has been more or leſs epidemic almoſt every year, in nearly all the ſea-ports of the United States.

"The years 1699 and 1717 are the dates of the two firſt attacks of the ſmall-pox in Charleſton. In both it proved fatal to a conſiderable proportion of the inhabitants. It returned in 1732, but effectual care was taken to prevent its ſpreading. In the year 1738 it was imported in a Guinea ſhip, and ſpread ſo extenſively that there was not a ſufficiency of well perſons to attend the ſick, and many periſhed from neglect and want. There was ſcarcely a houſe in which there had not been one or more deaths. Dr. Mowbray, ſurgeon of a Britiſh man of war then in the harbour, propoſed inoculation; but the phyſicians oppoſed it. Col. Beale, who was informed of the ſucceſs of inoculation in Boſton, had his family inoculated. Mr. Philip Prioleau was the firſt perſon in Charleſton who ſubmitted to the operation. The ſucceſs which attended this firſt experiment encouraged ſeveral others to follow the example. The diſeaſe ſoon after abated.

"About the beginning of the year 1760, the ſmall-pox was diſcovered in the houſe of a pilot on White Point. Guards were placed round the houſe, and every precaution taken to prevent the ſpreading of the diſeaſe, but in vain. When the perſons firſt infected at White Point were either dead or well, the houſe in which they had lain was ordered to be cleanſed. In doing this a great ſmoke was made, which being carried by an eaſterly wind, propagated the diſeaſe extenſively to the weſtward, in the line of the ſmoke. Inoculation was reſolved upon, and became general. Fifteen hundred perſons are ſaid to have been inoculated in one day; but it is alſo ſaid that five hundred of theſe died. This mortality, though great, was comparatively leſs than what had taken place in the year 1738.

"In the year 1763 the ſmall-pox again became epidemic; but as there were few to have it, and inoculation was generally adopted, its ravages were not extenſive. For ſeventeen years after the year 1763, the ſmall-pox was ſeldom or never heard of. During the ſiege of Charleſton it was introduced, and immediately after the ſurrender of the town, on the 12th of May, 1780, a general inoculation took place. As the

cool regimen was then univerſally adopted, the diſeaſe paſſed over without any conſiderable loſs or inconvenience. Since that time no pains have been taken to exclude the ſmall-pox. To inoculate for it is a thing of courſe, and the diſeaſe has ceaſed to be formidable.

"For eighty or ninety years after the firſt ſettlement of South-Carolina, the practice of phyſic was almoſt entirely in the hands of Europeans: among theſe were ſeveral able phyſicians, who poſſeſſed an accurate knowledge of the diſeaſes of our country. In this claſs Dr. Moultrie, the elder, was eminently diſtinguiſhed. He died twenty-eight years ago, after having been at the head of his profeſſion for more than forty years. He was the idol of his patients. They who had him once, could not be ſatisfied, in caſe of need, without having him again. So great was the confidence repoſed in his judgment, that they who were uſually attended by him, preferred his advice and aſſiſtance, even on the feſtive evening of St. Andrew's day, to that of any other profeſſional man in his moſt collected moments. He poſſeſſed excellent talents for obſervation, and was wonderfully ſagacious in finding out the hidden cauſes of diſeaſes, and adapting remedies for their removal. His death was regretted as a great public calamity. Several of the ladies of Charleſton bedewed his grave with their tears, and went into mourning on the occaſion. The year after his deceaſe was diſtinguiſhed by the deaths of ſeveral women in child-birth. While he lived, they thought themſelves ſecure of the beſt aſſiſtance in the power of man, or of art, in caſe of extremity. In loſing him, they loſt their hopes. Depreſſing fears ſunk their ſpirits, and, in an unuſual number of caſes, produced fatal conſequences.

"In that early period of American medical hiſtory, which was before Dr. Ruſh began his brilliant career as an author, there were more experiments made, more obſervations recorded, and more medical writings uſhered into public view, by the phyſicians of Charleſton, than of any other part of the American continent. Dr. John Lining, of this city, favoured the public with a ſeries of judicious ſtatical experiments, perſeveringly conducted through the whole of the year 1740. He was one of the firſt experimenters in the novel ſubject of electricity, with which he became acquainted, in correſponding with Dr. Franklin, ſoon after the diſcoveries of that celebrated man had aſtoniſhed the philoſophers of both the old and new hemiſphere. Dr. Lining, alſo, in the year 1753, publiſhed an accurate hiſtory of the American yellow

fever, which was the firſt that had been given to the public from our continent.

"Dr. Lionel Chalmers made and recorded obſervations on the weather for ten ſucceſſive years, that is, from 1750 to 1760. The ſame able phyſician furniſhed a particular account of the opiſthotonus and tetanus, which was communicated to the Medical Society in London, in the year 1754, and afterwards publiſhed in the firſt volume of their Tranſactions. He alſo prepared for the preſs an account of the weather and diſeaſes of South-Carolina, which was publiſhed in London, in the year 1776; but his moſt valuable work was an eſſay on fevers, printed in Charleſton, in the year 1767. In this he unfolded the outlines of the modern ſpaſmodic theory of fevers. Hoffman had before glanced at the ſame principles; but their complete illuſtration was reſerved for Cullen, and laid the foundation of his fame.

"Dr. Garden, about the year 1764, gave to the public an account of the virtues of pink-root, and, at the ſame time, gave a botanical deſcription of the plant. This truly ſcientific phyſician was much devoted to the ſtudy of natural hiſtory, and particularly of botany, and made ſundry communications on thoſe ſubjects to his philoſophical friends in Europe.

"In compliment to him, the greateſt botaniſt of the age gave the name of Gardenia to one of the moſt beautiful flowering ſhrubs in the world.

"William Bull was the firſt native of South-Carolina who obtained a degree in medicine. He had been a pupil of Boerhaave; and, in the year 1734, defended a theſis 'De Colica Pictonum,' before the Univerſity of Leyden. He is quoted by Van Swieten, as his fellow ſtudent, with the title of the learned Dr. Bull.

"John Moultrie was the firſt Carolinian who obtained a medical degree from the Univerſity of Edinburgh, where, in the year 1749, he defended a theſis 'De Febre Flava.' Between the years 1768 and 1778, ten more natives obtained the ſame honour. Three of theſe, viz. Drs. Fayſſoux, Harris and Chanler, have been preſidents of our ſociety.

"Since the revolutionary war, the number of native ſtudents has very much increaſed. Among them are ſeveral young men of great hopes. If we may judge of their profeſſional ſkill by the ſpecimens they have already given, we may augur well to our country. It is no inconſiderable evidence of the increaſing proſperity of South-Carolina, and of

the progreſs of medical knowledge therein, that at the cloſe of the eighteenth century, in the ſpace of four years, fifteen of its native ſons, viz. Johnſon, Alſton, Huger, North, Prioleau, Brailsford, Bellinger, Akin, Baron, Gough, Glover, Hunſcombe, Screven, Smith and Tidyman, have been admitted to the degree of Doctors of Medicine, which is treble the number of all the Carolinians who had attained that honour, thirty-two years ago, at the expiration of one hundred years from the firſt ſettlement of the province, in 1669. In that long period of our infancy, the whole number of native graduates was only five, viz. Bull, Moultrie, Chanler, Caw, and Fayſſoux."

Dr. Ramſay mentions, with great reſpect, LINING, CHALMERS, MOULTRIE, GARDEN and WALTER, South-Carolinians, who have eminently contributed to the enlargement of our knowledge of medicine and natural hiſtory. We barely obſerve, but without complaining, that we have looked in vain for the name of CATESBY. The author exhorts the members to loſe no opportunity of improving their profeſſion; adviſes an accurate analyſis of rice, the ſtaple commodity of the State; the inveſtigation of the qualities of indigenous vegetables; and the erection of a diſpenſary for the relief of the poor.

He concludes by exhorting the phyſicians of Charleſton to accept ſeats in the City Council, that they may aſſiſt in amending the Police, particularly as reſpects the prevention of peſtilential diſtempers. It is a remarkable fact, that very few inſtances exiſt, in any of the American ſea-ports, where phyſicians are members of municipal bodies. By generally withdrawing themſelves from juries, from moſt town and ward offices, and from the militia, medical gentlemen are looked upon as exempts from the active ſervices of ſociety, and excluſively devoted to the purſuit of their profeſſion. This appears, in many inſtances, to be carried too far; for thereby the public, in a conſiderable degree, loſes the benefit of their extra-profeſſional counſel, and their profeſſion itſelf is deprived of much of the influence it ought to poſſeſs in the community. Believing, as we do, civic duties and relations are as binding upon phyſicians as upon other claſſes of citizens, we think they ſhould exert themſelves to perform, at leaſt, their proportional part of the public labour; and, in doing ſo, be enabled to participate an equal ſhare of public uſefulneſs and honour.

ART. III. *An Addreſs to the Philadelphia Medical Society, on the Analogies between Yellow Fever and True Plague. Delivered, by Appointment, on the 20th February, 1801. By* Charles Caldwell, *M. D.* 8vo. pp. 44. *Philadelphia.* T. *and* W. Bradford. 1801.

WHEN coloniſts from Europe firſt eſtabliſhed themſelves on the eaſtern ſhores of North-America, it was natural, and, in many inſtances, indeed, during ſome time, neceſſary for them to derive their ſupplies, both of food and of knowledge, from the country whence they emigrated. The proviſions which they brought with them might ſoon be conſumed, and additional quantities imported from their former homes. But, in due ſeaſon, the plants and animals brought with them, by thriftineſs and increaſe in their new ſituations, furniſhed an adequate ſtore of nouriſhment; and, in addition to theſe, the indigenous productions of the ſoil itſelf on which the adventurers had fixed their abode, likewiſe contributed ſomething toward their ſubſiſtence. Soon, therefore, would they become independent of their parent land for the neceſſaries of *bodily* ſupport.

Far otherwiſe was it with regard to the *mind.* Having little leiſure, from the preſſure of immediate wants and exertions, to cultivate their underſtandings, they languiſhed through neglect of culture, or ſickened for lack of knowledge, *the true phyſic of the ſoul.* Or if, at length, information was ſought after and obtained, it was generally through the medium of the publications, diſcourſes and converſations faſhionable in the mother country, and tinctured, of courſe, with the prejudices and errors of every ſort which were prevalent there. To this ſource can we trace language and its dialects, religion and its rites, government and its forms, manners and their tendencies, cuſtoms and their peculiarities, with much of the habits, taſte and genius of the different tribes of our people. From the ſame origin came the literature and ſcience which reigned in their infant ſchools and ſeminaries, where frequently the books that were read, and the inſtructors who preſided, were imported from foreign dominions.

Great as this influence was, it had in no department of knowledge more ſway than in the profeſſion of phyſic. Many of the practiſers of this art were natives of Europe. Of the deſcendants of the coloniſts, many were ſent to Holland, or England, or Scotland, for education: of thoſe who remained

at home, the greater part, either directly or indirectly, gleaned ſomething from the books in vogue; and as they peruſed the writings of Pitcairn, Allen or Salmon, made up their minds accordingly.

But this ſtate of dependence was not to continue for ever. The ſevering of the political connection between this country and Europe was a glorious event. Not only an eſcape from the tyranny of political and theological, but of literary and ſcientific domination, was effected. Taught to think and act for themſelves on governmental, military and commercial ſubjects, the American people alſo began to aſſert independence in matters of a philoſophical nature.

A peſtilential ſickneſs, one of the moſt ſerious calamities that ever fell upon civilized ſociety, has ſtimulated phyſical inquiries, and exerciſed medical talents, to an extraordinary degree. Under the deſolating influence of this diſtemper, the Americans grew vigilant, and ſuffered none of its accompaniments to eſcape their notice. They beheld its riſe, progreſs and decline, year after year, under circumſtances the moſt favourable to its inveſtigation. Their feelings, their hopes and their intereſts, all prompted them to be correct in their obſervations, clear in their narratives, and fair in their reaſonings. A candid ſpirit of inquiry, like Ariadne's thread, led them through the labyrinth, and enabled them, on their return, to make a true report of what they had witneſſed, inſtead of having been, like their predeceſſors, bewildered and loſt in its mazes.

They declared, as the reſult of their experience, that the yellow fever, which they had ſo often ſeen, was a plague, whoſe peſtilential quality, however remotely dependent upon the general conſtitution of the atmoſphere, as modified by climate and ſeaſon, was particularly occaſioned by poiſonous exhalations from certain ſorts of organized ſubſtances in a corrupting condition; that beef, and fiſh, and hides, and human fæces, putrefying in hot weather, had often afforded ſuch a deleterious air, by which many perſons had been diſeaſed, and many killed; that this ſeptic effluvium was moſt noxious near the place of its production, and could not be formed, even from materials which contained its elements, but by the joint operation of moderate degrees of heat and moiſture. They alſo declared their conviction, that febrile diſtempers of all denominations, from quartan agues to Syrian plagues, were excited by the ſame general cauſe, variouſly modified in its

ſtrength and virulence, and in reſpect to the food, way of life, habits, temperaments, dreſs, prediſpoſition, and part of the conſtitution to which it was applied; that this cauſe being not the offspring of living vaſcular ſecretion, but of common putrefactive diſſolution, and therefore not a contagion, would not produce a contagious diſeaſe; and that, of courſe, not one of the diſorders ariſing from this general or common ſource of miſchief was (in the proper ſenſe of the word) contagious, or liable to ſpread from man to man in any other manner than as a putrid and ſickening vapour might be engendered in the diſcharges of the body before, and in the body itſelf after death. And they further became ſatisfied, that if this plague of America was not contagious, the plague of Aſia ought not to be contagious; that the yellow fever often aroſe from local cauſes on board merchant-ſhips and veſſels of war, and was thence erroneouſly ſaid to be brought from places beyond the ſeas; and, by parity of reaſoning, the plague of the weſtern Mediterranean ports was not introduced from the Archipelago and Levant, but engendered on ſhip-board, through improper regulations there, either during their voyages, or rather during a quarantine, or forty days detention at the place of their arrival; and, conſequently, that this miſconception had given riſe to many unneceſſary and burthenſome reſtraints upon commerce, to the great reproach of legiſlation, and the diſgrace of ſcience.

Dr. Caldwell is one of thoſe whoſe underſtanding has been long exerciſed in contemplating the appearance and diſappearance of the peſtilential diſtempers of Philadelphia. They have repeatedly ariſen, taken their courſe, and died away before his eyes, under circumſtances the moſt favourable for obſervation and remark. And the piece now under conſideration is a reſpectable teſtimonial of the care he has taken to gather facts, and of the ability he poſſeſſes to reaſon upon them. He expreſſes his opinion, that under the term "peſtilence" ſhould be included all diſeaſes which ariſe from a vitiated and malignant atmoſphere. Plague and yellow fever ariſe from this cauſe, and conſtitute two of the leading modifications of peſtilence. Being certainly promoted by putrid exhalations, officers of police ought to be much leſs ſolicitous to prevent their introduction from abroad, than to guard againſt their local and inteſtine engendering among ourſelves. Diſbelieving the contagiouſneſs both of the one and the other of theſe diſtempers, he conſiders the importation of their ſpe-

cific poiſon from countries beyond the ſeas, and their propagation by means of it in our own, as a groſs and vulgar miſtake.

The publication having been made at the requeſt of the Medical Society, Dr. C. has dedicated it to the members.—After a modeſt and handſome exordium, he ſtates his ſubject, and paſſes ſome pointed ſtrictures upon the author of the Treatiſe on Plague and Yellow Fever, which was reviewed in our vol. iii. p. 373 & ſeq. He afterward, in page 7, proceeds to enumerate the analogies thus:

"The plague of Aſia, like the yellow fever or peſtilence of our own country, is a diſeaſe which delights in the devaſtation of populous cities. Perhaps neither of theſe calamities has ever been known to originate, as an epidemic, in villages or country ſituations; nor do they oftentimes extend to ſuch places, even in times of their moſt general prevalence. It is only in the artificial and vitiated atmoſpheres of large cities that they are able to find a ſufficiency of their proper nouriſhment.

"True plague, when it has prevailed in the cities of Europe, has always made its appearance about the cloſe of ſpring, or in the courſe of the ſummer—has reached its ſummit in the autumnal months, and declined, or wholly diſappeared, on the commencement of cold weather. But ſuch is known to be, in like manner, the courſe of the yellow fever or peſtilence of America. To the generation and propagation of both theſe diſeaſes, therefore, a warm atmoſphere is no leſs neceſſary than the filth and crowded population of large cities. Even when committing their heavieſt devaſtations, a few nights of cold weather, particularly if it be accompanied by froſt, will check them as certainly, and almoſt as ſpeedily, as it will the vegetation of tropical plants. I know of no inſtance on record of either of theſe diſeaſes having prevailed, to any extent, throughout the winter of high latitudes.

"It is a circumſtance alike common to plague and yellow fever, that they occur epidemically, in places ſubject to them, only at certain irregular periods, or after indefinite intervals of time. Fortunately, theſe deſtroyers of the human race do not appear as the regular endemics of any ſituation. Though ſummer and autumn are their native ſeaſons, yet theſe ſeaſons are not alone ſufficient to produce them, unleſs they be aided by a peſtilential conſtitution of atmoſphere.

"Epidemic plague baniſhes from around it every other

form of febrile diſeaſe, and reigns itſelf the ſolitary tyrant of the place where it prevails. It would be ſuperfluous to offer teſtimony in confirmation of this fact. Moſt authors on peſtilence, who have written from obſervation, bear witneſs to its truth. But the ſame thing is true with regard to the epidemic ſtate of yellow fever. Taking poſſeſſion of the atmoſphere, it expels or ſuffocates all other deſcriptions of fever, and creates for itſelf a diſmal ſolitude. During the late autumns in which it prevailed in this place, it is known by our practitioners to have ſuffered no rival diſeaſe to approach it.

"Plague and yellow fever are alike remarkable for being preceded, followed, or accompanied in adjacent places, either by new diſeaſes, or by an increaſe in the frequency, but more particularly in the malignity of common diſeaſes. Since the year eighty-nine [1789?], this country has been ſeveral times viſited by epidemic catarrh, and in ſome places by an epidemic cynanche, diſeaſes which had not before occurred to ſuch extent, within the memory of our oldeſt citizens. Since the ſame period, the common diſeaſes, in different parts of the United States, have undergone ſuch a ſtriking change, as to call for a different and much more energetic mode of treatment. That ſimilar phenomena are connected with the prevalence of plague in Europe, and in the Eaſt, we learn from many works on the ſubject, particularly from that of Dr. Patrick Ruſſel, and from Mr. Webſter's excellent 'Hiſtory of Epidemic and Peſtilential Diſeaſes.'

"It belongs in common to plague and yellow fever to be attended with moſt fatality on their firſt occurrence. It is a melancholy truth, that theſe diſeaſes ſeldom fail to hurry to the grave a great proportion of their earlier ſubjects. As they become more general, but eſpecially when they are on the decline, individual caſes, laying aſide their malignity, prove much more manageable, and may be generally conducted to a favourable iſſue. Theſe circumſtances are probably attributable to a threefold cauſe.

"1. Thoſe firſt attacked by, would appear to be moſt ſtrongly prediſpoſed to, the above diſeaſes, and therefore to be moſt likely to have them in a violent degree.

"2. In this early ſtate of things, an ignorance of the dangerous nature of their complaints, prevents patients from being ſufficiently prompt in applying for medical aid.

"3. On the firſt occurrence of an epidemic, phyſicians themſelves, neither acquainted with its character nor aware of its

malignity, generally fail to treat the early caſes of it with ſufficient boldneſs.

" Neither plague nor yellow fever can prevail at any time, nor in any place, which is not calculated to give riſe to an abundance of putrid exhalation. Whatever is inimical to the origin of ſuch exhalation, proves equally inimical to theſe complaints. But the production of this poiſonous gas is particularly prevented by three cauſes, namely, cold, great humidity, and exceſſive heat, accompanied by an exceſſive aridity of the atmoſphere.

" The hoſtility of the firſt of theſe cauſes to the diſeaſes in queſtion, has been already mentioned; that of the two latter ſhall be treated of in a ſubſequent part of my addreſs.

" Thus far on the analogies derived more immediately from the origin of plague and yellow fever. In the phenomena connected with their propagation, they exhibit no leſs of a kindred nature.

" Though they have both ſomewhat the appearance of ſpreading by contagion, their progreſs is certainly too rapid to depend on a cauſe of ſuch limited powers. Allow me to call your attention to a palpable inconſiſtency (not to ſay an abſolute contradiction) which exiſts in the ſtate of common opinion on this ſubject.

" Plague and yellow fever are acknowledged by every one to be much leſs contagious than ſmall-pox, becauſe they attack, with much leſs certainty, perſons expoſed for a ſhort time to their influence. Nor are they ſuppoſed to be communicated to ſo great a diſtance as ſmall-pox. Notwithſtanding this, they ſpread with nearly tenfold its rapidity. In the courſe of a few weeks they will overrun an extent of city, which it would ſcarcely pervade in twice as many months. Here, then, admitting plague and yellow fever to ſpread by contagion at all, they would appear to be both more and leſs contagious than the ſmall-pox. But this is a poſition which even the advocates of contagion will reject. Perhaps the only method of removing the contradiction is to admit, that the two former diſeaſes are not propagated by contagion at all, but by the more powerful medium of a vitiated atmoſphere. But more of this hereafter.

" It is a fact notorious in the United States, that if a patient in the moſt malignant ſtate of yellow fever be removed from the city to the country, he may there be viſited and nurſed, without endangering the health of his phyſicians or attendants. But we learn from Proſper Alpinus, Ruſſell, Mariti, Sonnini,

and various other writers, that the ſame thing is true with regard to the peſtilence of the Eaſt. When perſons ill of that diſeaſe are conveyed from a place where it is epidemic, to one where it is not, they may be approached and attended without hazard.

"True plague, therefore, exhibits no unequivocal marks of contagion, when removed without the limits of a malignant atmoſphere. That calamity is believed to be, for the moſt part, introduced into Syria and Egypt from Conſtantinople. Yet, in common years, when no peſtilential ſtate of the elements prevails, perſons in all ſtages of it arrive from the latter in different parts of the former places, without communicating infection to any one.

"The phyſicians of America have not now to learn, that certain claſſes of perſons are much more liable than others to be attacked by yellow fever. But this is in like manner true with reſpect to the oriental peſtilence: and it is not a little remarkable, that theſe two diſeaſes manifeſt a predilection for perſons and habits of the ſame deſcription.

"Does yellow fever attack and deſtroy men rather than women? So does peſtilence.

"Does the former ſelect, as its victims, the robuſt, the healthy, and the young, rather than the weak, the infirm, and the aged? Such is alſo the caſe with the deſtroyer of the Eaſt. Perſons turned to their ſeventieth year have, comparatively ſpeaking, but little to apprehend from that dreadful calamity.

"Does yellow fever ſpare infants and children rather than adults? And does not a ſimilar diſcrimination characterize the deſolating courſe of peſtilence?

"Finally, do not both theſe diſeaſes either entirely paſs over, or but ſlightly affect, the frugal and the ſober, while they attack more certainly, and more certainly deſtroy, the luxurious and the intemperate?"

Dr. C. then goes on to note the concomitant ſickneſs, from the ſame cauſe, of dogs, cats, horſes and kine, and even of birds and fiſhes. He enumerates ſome peculiarities in natural phenomena during peſtilential ravages, ſuch as irregularities in the weather, the diſappearance of birds, the multiplication of inſects, and the luxuriance and ſickline ſs of plants. He next recites the *pathological* characters of the two diſeaſes, and ſhows that they have a ſtrong and marked reſemblance. In their *decline* and *termination* there is as ſtriking a ſimilitude. Local conſiderations concerning Egypt and Syria are brought forward to illuſtrate and enforce the argument; wherein he

boldly aſſigns the reaſons which have inclined him to conſider the plague of the eaſtern world "as propagated not by contagion, but through the medium of a vitiated atmoſphere."—In doing this he varies widely from the common doctrine of the ſchools. But in this caſe we conſider a departure from ancient prejudice and authority highly neceſſary. And we applaud the intrepidity of ſpirit no leſs than the juſtneſs of concluſion manifeſted in this well-timed and well-managed addreſs. We wiſh it may be extenſively read; for, to the young and unlearned, it will exhibit an inſtructive parallel between two diſtempers heretofore thought very diſſimilar; and to thoſe of more mature years, whether they coincide in ſentiment with the author or not, it will preſent an ample theme for reflection.

According to the plan propoſed in the title-page, the orator ſhould have concluded here, and we really wiſh he had made this his peroration. We ſhould then have had no ground of difference with him on any point of moment. But, animated with his ſubject, he undertakes the diſcuſſion of another queſtion, in which, we believe, he is leſs fortunate and ſucceſsful. Not content to eſtabliſh the general identity of cauſe and chracter between plague and yellow fever, he goes a ſtep further, and attempts to prove the radical diſſimilarity of *plague* and *jail* or *typhus fever*. It had been aſſerted by ſome, that peſtis was nothing more than the higheſt grade, or moſt exalted form of typhus. Dr. C. differs entirely from them, and declares typhus to be "perfectly independent of the putrefactive proceſs," and that a "vitiated ſecretion" (ſpecific contagion we ſuppoſe) "is all that it requires for its origin and propagation." We have no evidence of ſuch a morbid ſecretion. On the other hand, it appears more probable that the poiſonous matter exciting typhus is formed from human excretions, ſweat, urine, &c. by means of the chemical changes they undergo, like other inanimate ſubſtances. This is formed commonly within doors, in cloſe chambers, and inheres particularly in bedding and clothing, not waſhed and alkalized as often as they ought to be. This explains its more common prevalence in the abodes of poverty and filth, and its connection with petechiæ produced by the local action of the virus upon an unclean ſkin. We are further inclined to believe, that the difference of the typhoid ſymptoms is no evidence of a difference of exciting cauſe; becauſe it would be very extraordinary indeed if the morbid virus, acting upon emaciated and impoveriſhed conſtitutions, ſhould produce the ſame effects

which it might be capable of working in thoſe which are full fed, and in a highly vigorous ſtate.

We conceive that much of the difficulty of this ſubject may be explained, by calling to mind that the heat of the human body, operating upon its own excretions too long retained and accumulated in bed and body clothes, by habits of uncleanlineſs, in cloſe and filthy apartments, during the cooler ſeaſons of the year, may probably produce effects analogous to thoſe of high atmoſpherical temperature upon dead animal and vegetable matters during the ſummer and autumn. Particular conſtitutions of the air favour each of theſe proceſſes; but they ſeem likely to exert a more deciſive influence in the latter than in the former. Hence typhus may occur, often during a cool ſeaſon, in any family where cleanlineſs and ventilation are neglected. But the peſtilential epidemics of the ſummer and autumn appear always to require the aid of a general unſalutary condition of the atmoſphere.

ART. IV. *Tranſactions of the American Philoſophical Society, &c. Part* iv.

[Continued from page 182.]

Hints relative to the ſtimulant Effects of Camphor upon Vegetables. By Benjamin Smith Barton, *M. D.*

THE experiments made on this ſubject induce Profeſſor Barton to think that camphor exerts a greater ſtimulant effect upon plants than any other ſubſtance with which he is acquainted. If the expenſiveneſs of it ſhould forbid us to employ it as a manure upon a large ſcale, ſtill the author ſuggeſts, that "a few grains of camphor, acting as a cordial, will revive a drooping plant, will increaſe its beauty, and prolong its exiſtence"—objects of no mean importance in the eye of the floriſt. After ſome experiments on the comparative ſtimulating effects of camphor and nitre, Profeſſor B. concludes that the reſult favours the idea, that camphor is a more wholeſome ſtimulant than nitre.

Experiments and Obſervations on Land and Sea Air. By Adam Seybert, *M. D.*

In this paper we are preſented with a variety of eudiometrical experiments on land and ſea air, in many different ſituations.

Dr. Seybert believes that the air over a large body of water is always purer, *cæteris paribus*, than that of the adjoining land, owing, as he conjectures, to a decompoſition which the water may ſuffer from the action of the ſun's rays; and likewiſe, in part, to its abſorbing many foreign matters, which, on land, are more or leſs intimately mixed with the air in a mechanical way.

As to the ſtate of the air in different ſituations on land, Dr. S. adopts the concluſion of all the more reſpectable eudiometrical experimenters, that the reſults vary but little. And he is diſpoſed to admit the opinion of Fontana, that "the difference in the purity of the air, at different times, is much greater than the difference between the air of the different places."

Obſervations intended to favour a ſuppoſition that the black Colour (as it is called) of the Negroes is derived from the Leproſy. By Dr. Benjamin Ruſh.

The opinion ſupported in this paper is ſuppoſed to be rendered probable by the following facts and analogies. 1ſt. That the leproſy is accompanied, in ſome inſtances, with a black colour of the ſkin. 2dly. That the preternatural whiteneſs of the ſkin, often obſerved in leproſy, bears a ſtriking reſemblance to that morbid condition of the ſkin, among ſome negroes, diſtinguiſhed by the name of *albinos*, which has been ſuſpected to be a modification of leproſy. 3dly. By the blended appearance of white and black ſometimes obſerved in caſes of leproſy. 4thly. By the ſimilarity of the morbid inſenſibility of the nerves, in leproſy, to the conſtitutional inſenſibility common among negroes. 5thly. By the ſtrong propenſity to venereal gratifications, which equally diſtinguiſhes the leprous and the negro conſtitution. 6thly. By the big lip and flat noſe of the negroes, which are likewiſe ſymptoms of the leproſy. 7thly. By the analogy between the woolly hair of the negroes, and the trichoma or plica polonica of the Poles, which is a ſymptom of leproſy.

An Inquiry into the comparative Effects of the Opium Officinarum, extracted from the Papaver Somniferum, or White Poppy, of Linnæus; and of that procured from the Lactuca Sativa, or common cultivated Lettuce, of the ſame Author. By John Redman Coxe, *M. D. &c.*

Dr. Coxe's firſt experiments on theſe two ſpecies of opium were made on ſolutions of them in water, alcohol, and in a

mixture of equal parts of both. And the reſults convinced him that the two ſpecies are very ſimilar in their qualities.

After ſubjecting them to a variety of chemical teſts, he ſtill found the ſimilarity to hold good in a remarkable degree.

By introducing the watery ſolutions and ſpirituous tinctures of each ſpecies of opium under the ſkin, and in contact with the muſcles of frogs—into the ſtomach and rectum, and into the cavity of the abdomen, of the ſame animal—and likewiſe by bringing them into contact with the eyes and brain—he diſcovered that both ſpecies poſſeſs nearly the ſame powers.

Experiments and Obſervations on the Atmoſphere of Marſhes. *By* Adam Seybert, *M. D.*

It is the object of this paper, firſt, to determine whether or not the air of marſhes differs from that of other ſituations; ſecondly, to aſcertain what are the cauſes of the differences which are found to exiſt; and, thirdly, to make obſervations and remarks.

I. It appeared from a number of experiments on air, obtained immediately as it was diſengaged from the marſhy ſoil, that carbonic acid gas enters largely into its compoſition—that hydrogen gas is an ingredient in it—and that no oxygen gas is preſent.

It was found that air, obtained at the height of ſeveral feet above marſhes, contains little or no hydrogen gas—that the proportion of carbonic acid gas is pretty conſiderable—and that a large quantity of oxygen enters into its compoſition. Hence the author is induced to believe that the air *above* marſhes is not conſiderably different in its properties from the common atmoſphere in other ſituations, where animals reſpire with eaſe, and enjoy perfect health, except the proportion of carbonic acid gas being greater, the gravity of which permits it to riſe only to an inconſiderable height.

II. What are the cauſes of the peculiarities found to exiſt in the air of marſhes?

Putrefaction of the animal and vegetable matters upon the ſoil of marſhes is conſidered by the author as the great cauſe of the changes obſerved to exiſt in the air of them. That this is ſo he infers from the following circumſtances:—"Marſhes have no noxious influence during the winter ſeaſon. They cauſe diſeaſe when the circumſtances are preſent which promote putrefaction; as a proper degree of heat, a due quantity of moiſture, and the contact of atmoſpheric air, or ſubſtances capable of affording oxygen, as water."

Many experiments to aſcertain the action of mud, obtained from marſhes, on atmoſpheric air, were made at different times, by our author, during the ſummer and autumnal months of the years 1796 and 1798. He obſerves that theſe "experiments teach us that mud vitiates the atmoſphere in a very powerful manner. They alſo enable us to account for the preſence of the elaſtic fluid forming the atmoſphere of marſhes. It appears that the carbon of the mud unites with the oxygen of decompoſed water, and forms the carbonic acid gas, whilſt the hydrogen gas is ſet at liberty."

III. The remainder of the paper is devoted to inferences and concluding remarks.

In order to arrive at any juſt concluſion concerning the effects of marſhes on the atmoſphere, the author recommends the inveſtigation of their compoſition. They are found to conſiſt of more or leſs water—of different proportions of dead animal and vegetable matters—and of the earthy ſubſtances compoſing the original ſoil. Heat, moiſture, the contact of atmoſpheric air, and reſt, are circumſtances attendant on marſhy ſituations during the unhealthy ſeaſons. From this aſſemblage of materials and agents, it might be ſuppoſed *a priori*, our author obſerves, that the following effects would reſult: 1. That hydrogen gas would be diſengaged. 2. That oxygen, combining with carbon, would form the carbonic acid gas. 3. That azote would unite with a portion of hydrogen, and thus produce ammonia; whilſt another portion of it would, during its combination with oxygen, form the nitric acid. And, 4. That when ſulphur and phoſphorus were preſent, they, with hydrogen, would form the ſulphurated and phoſphorated hydrogen gaſes.—Dr. Seybert conſiders the preſence of hydrogen gas, and of carbonic acid gas, in the atmoſphere of marſhes, as eſtabliſhed by undoubted proofs. Ammoniacal gas he ſuppoſes to be one of the products of putrefaction; but he doubts the preſence of it in the air of marſhes, on account of its ready combination with water, and its proneneſs to unite with carbonic acid gas into a carbonate of ammoniac. As to nitric acid, though doubtleſs a product of putrefaction, he concludes that it is immediately abſorbed by the neighbouring waters. He expreſſes conviction, that ſulphurated and phoſphorated hydrogen gaſes do not exiſt in the air of marſhes.

On the whole, Dr. S. conſiders marſhes as neceſſary to keep the atmoſphere *in a proper degree* of purity: for not only the impure atmoſphere, but the too pure alſo, is de-

ſtructive to animals. And he imagines marſhes to have been formed by the Author of Nature in order to operate againſt the powers which vegetables and other cauſes poſſeſs of purifying the atmoſphere, ſo that the oxygen may exiſt in a proper proportion fit to ſupport animal life and combuſtion.

(To be continued.)

ART. V. *A Phyſiological Treatiſe, &c.* *By* Joſeph Young, *M. D.*

[Continued from page 285, and concluded.]

HAVING already examined that part of this work which reſpects aſtronomy, we now proceed to the ſecond part, which treats of a number of phyſiological topics, and delivers rules and precepts for the management of many important diſeaſes.

In the firſt place the author conſiders the earlieſt ſtage of life in the incipient animal, and ſhows the means by which circulation is performed in the firſt rudiments of it, before the veſſels are completely organized.

The animal ſpirits, duly prepared, according to our author, are the plaſtic means by which the incipient embryo is produced out of an unorganized maſs of animal gluten. He conſiders them as the link of connection between the ſoul and body, operating through the mechaniſm of the nerves and living ſolids. He ſuppoſes them to be ſecreted in the brain—to conſiſt of the moſt highly attenuated parts of the lymph, united with the electric matter—to be the ſpring of heat, life, ſenſation and muſcular motion in the animal ſyſtem—and the ſole cauſe of circulation in the lacteals, lymphatics, &c.

In the next place the author proceeds to treat of the *vis medicatrix naturæ*—of his doctrine of *vinous fermentation*—and of *digeſtion* by means of *alimentary fermentation* and the gaſtric liquor. He likewiſe offers his opinions concerning the organization of the animal ſyſtem by fluids—concerning cold and its effects on animals—and concerning the generation of heat in reſpiring animals.

Having laid down his general doctrines on theſe ſubjects, he then goes on to treat of particular diſeaſes. He gives directions for the management of *frozen limbs*, by immerſing them in ſpring-water for ſome time before the application of warmth. His theory and practice in *dyſentery* are next pre-

ſented to the reader. In the management of *cancers* he places much dependence upon the root of the ſharp-pointed dock, uſed internally by way of infuſion, and externally by application of its expreſſed juice; and he alſo thinks well of arſenic, and the phytolacca, or poke.

Cynanche maligna is the next ſubject of diſcuſſion. As a ſpecimen of the work, we quote the following practical directions concerning that diſeaſe:

"If the diſeaſe is at firſt attended with great heat and thirſt, add two table ſpoonfuls of the beſt yeaſt to a pint of a ſtrong infuſion of good ground barley malt (in boiling water), after the infuſion gets to be about milk warm, of which the patient may take a wine glaſs full every hour while it is fermenting; and, at the ſame time, uſe the following gargle:—Take the root of the yellow or ſharp-pointed dock half an ounce, Virginia and Seneca ſnake-roots of each one drachm and an half, Peruvian bark ſix drachms, myrrh and ſal ammoniac of each two drachms, ſal nitre one drachm, honey three ounces; infuſe the roots and bark in one pint and an half of boiling water, rub the myrrh in a mortar with two ounces of camphorated ſpirit of wine, until it is pretty well diſſolved, and when the infuſion is milk warm add the ingredients, together with two table ſpoonfuls of good yeaſt; and while it is fermenting the patient ſhould gargle it in his throat frequently; and if the tonſils are ulcerated they ſhould be frequently waſhed with a ſmall ſwab, made by rolling old ſoft linen about a ſmall ſtick. And when the throat is very ſore pour boiling water on the inſide bark of the red elm until it grows ſlimy, and hold ſome of it frequently in the mouth, and gargle it, which will yield great relief.

"When the violence of the fever has abated, if there are yet evident ſigns of putrefaction remaining, take Peruvian bark ſix drachms, Virginia ſnake-root and contrayerva of each two drachms, bruiſe them in a mortar, and infuſe them in a pint and an half of boiling water, and when it is milk warm ſtrain it off, and add two ounces of ſugar and two table ſpoonfuls of yeaſt; and while it is in briſk fermentation let the patient take a wine glaſs full every ſecond or third hour.

"But in the country, where theſe ingredients cannot be obtained, I am certain that a gargle, made by infuſing an ounce of the inſide bark of the wild cherry-tree, and half an ounce of freſh ſharp-pointed dock-root, bruiſed, in a pint of boiling water, and ſweetened with honey, and an infuſion of the cherry bark and wild alſpice, uſed in the ſame manner as

above directed, would anſwer a very valuable purpoſe; uſing the mucilage of the red elm bark, as above directed, to prevent irritation.

"Good Port wine and water, ſweetened with loaf-ſugar, may be taken to advantage, as a cordial, if the heat of the body is moderate."

The remainder of the work is devoted to cynanche trachealis—to ſmall-pox by inoculation—to yellow fever—to anaſarcous dropſy—to putrid fevers—to confluent ſmall-pox—to mania—to fiſtula lachrymalis—to fiſtula in ano—to ſcrophula—and to phthiſis pulmonalis.

In the treatment of putrid fevers Dr. Young relies greatly upon the virtues of yeaſt. In mania, after blood-letting, cathartics and emetics, he recommends a free uſe of camphor combined with vinegar by the mucilage of gum arabic, a mild and indulgent treatment, &c.

The work is concluded by the following ſentiments of philanthropy, which muſt impreſs every reader with reſpect for the venerable author.

"In the preceding ſtages of my life I have been actively employed in the ſervice of my fellow beings; but, being worn out and ſuperannuated, I offer them this little book, probably as my laſt tribute; and if its utility but equals my intention, it will add one more gratification to a perſon who wiſhes every created being every degree of happineſs and pleaſurable ſenſation which their beneficent Creator has fitted them to enjoy."

Medical and Philoſophical News.

DOMESTIC.

USE OF ALKALIES IN CANCERS.

Extract from a Letter of Dr. J. Barker to Dr. Mitchill, dated Portland (Maine), March 12, 1801.

"BESIDES the beneficial effects derived from the uſe of alkaline remedies in FEBRILE diſeaſes, I have, for a few years paſt, found them to be of ſingular efficacy in certain *ſurgical* caſes, as well as in other maladies. My experience ſeems to verify your important ideas concerning the proneneſs of animal matter to grow *acid* under certain circumſtances; as alſo the propenſity which the pus of ulcers has to abſorb oxygen, and to become *ſour*.

"Six years ſince, a peaſant in a neighbouring town was afflicted with painful cancerous tumours in his face, which diſcharged a very corroſive humour. Several of theſe tumours were extirpated with the knife; and mercury was employed both externally and internally, as well as cicuta, with a view of correcting this humour, and of diſpoſing the ſores to heal. The ſores, however, would not heal upon this plan. Indeed, the humour increaſed in degree of virulency and corroſive power, notwithſtanding that the external applications were reinforced with vitriols, lead, various unguents, and even *arſenic*.

"He remained for a conſiderable time in this forlorn condition, laid aſide all means, and conſidered his diſeaſe as irremediable.

"I then directed a ſtrong lixivium of wood-aſhes to be applied. This readily ſtopped the progreſs of the corroding humour, alleviated his pains, and allowed the ſores to heal. By this means the cancerous humour was ſubdued, and rendered harmleſs; ſo that, in a ſhort time, the ulcers healed, and a radical cure was effected.

"Since this event, it has become a common practice in this part of the country, not only among phyſicians, but

others, to eradicate cancers with a ſtrong ſolution of pot-aſh, as well as with the lixivium of aſhes.

"This lixivium is made by boiling a gallon of common ley down to a pint. A doſſil of lint, impregnated with this ley, and applied to the cancerous tumour, ſoon alleviates the pain, and converts it into a black lump, which, by a common digeſtive poultice, readily ſeparates from the ſound fleſh. The ſore then heals by a ſimple dreſſing.

"I have employed alkaline remedies in ſeveral caſes of *ſcrophula*, both externally and internally, with very great advantage."

ALKALINE REMEDIES IN SYPHILIS.

The prevailing opinions, for two or three years, in Europe, have been ſtrongly in favour of curing lues venerea by oxygen and acids. The virus of this formidable diſeaſe is confidently ſaid to be decompounded, or neutralized, or expelled from the body, or the conſtitution to be rendered proof againſt it, by means of remedies abounding with the principle of acidity in a very ſeparable ſtate. Of theſe, the Medical Repoſitory contains ample accounts. In America we cannot learn that the acid practice has, in any inſtance, alone wrought a cure of lues. It is inſufficient without the aid of quickſilver. The general rule of ſucceſsful management is to eradicate the ſyphilitic poiſon by means of mercury; and, in caſe the conſtitution of the patient wants oxygen, to preſcribe acids alſo. In ſome habits they will do good, in others no good at all, and in others certainly harm. For a radical cure of the diſeaſe, they are not to be relied on.—It is remarkable how practiſers run into extremes. No longer ago than 1786, the ſecond edition of Mr. PEYRILHE's octavo volume on a *new remedy* againſt venereal diſtempers was publiſhed at Montpellier, and reviewed in the *Journal de Medecine* of Paris, for the month of December that year. This new remedy was the VOLATILE ALKALI, on the antiſyphilitic virtue of which the author wrote an able diſſertation. He was Regius Profeſſor of Chemiſtry and Botany in the College of Surgery in Paris; and his book, which contains the account of his own experience with *ammoniac*, is allowed by the critics to be full of valuable and nice practical remarks. And yet this work is diſregarded in Europe, and, we may ſay, in America is almoſt wholly unknown.

ADULTERATIONS OF PEARL-ASH.

Extract of a Letter from Mr. William Henry, of Manchester, in Great-Britain, to Dr. Mitchill, concerning the Adulterations of American Pearl-aſh, dated February 2, 1801.

"It is now nearly thirty years ſince my father introduced ſome improvements into the proceſs for preparing magneſia, which were approved by the London College of Phyſicians, and publiſhed in their Tranſactions. Calcined magneſia being, at that time, a medicine extremely difficult to be procured genuine, and, conſequently, very little in uſe, my father, with the advice of ſome of the leading members of the College, undertook the manufacture, and has continued it to this day. About four years ago I joined him in the buſineſs, and have made conſiderable improvements in the proceſs (chiefly in the mode of purifying the alkali), which enable us to offer to the public an article of very ſuperior quality. There is one impurity, however, very frequently found in the alkali, which, of late years, has thrown conſiderable difficulties in our way; and which no one, of the almoſt numberleſs experiments I have made, has inſtructed us to ſeparate. This is the carbonate of lime, for which, in ſmall proportion, the carbonate of pot-aſh has ſo ſtrong an affinity, that no chemical expedient I am acquainted with can wholly detach it. This carbonate of lime may be diſcovered in an alkali in the following manner: Mix a ſolution of the beſt American pearl-aſh with one of ſulphate of magneſia; waſh the carbonate of magneſia very well, and expoſe it, in open day, to a very ſtrong heat for a ſufficient length of time. The calcined magneſia ought to be perfectly inſipid; but it will often be found to have a very pungent and cauſtic taſte; and on digeſting it with diſtilled water, a true lime-water will be obtained. Now, as the ſame ſulphate of magneſia, with a ſolution of pearl-aſh, from a different barrel, or a different manufacture, will afford a magneſia perfectly inſipid after calcination, and which imparts no impregnation to water, it is clear that the defect muſt have been in the alkali. The lime, contained in carbonate of pot-aſh, cannot, on account of its ſtrong affinity for carbonic acid, exiſt in the ſtate of pure lime, and muſt, therefore, be preſent in a carbonated form. On mixing the alkaline ſolution with one of ſulphate of magneſia, this carbonate is precipitated along with the carbonate of magneſia; and the impurity does not become perceptible to the taſte till the carbonic acid is expelled from both by calcination.

"Of late years this defect in pearl-aſhes has become almoſt univerſal; and during the laſt eight or ten months we have not been able to meet with any American pearl-aſhes fit for the manufacture of calcined magneſia. That the adulteration is not practiſed in this country, I ſatisfied myſelf by remaining ſome time in Liverpool, and trying ſamples from the warehouſes of the moſt reſpectable merchants there, whoſe aſſurances that the aſhes were really as imported I cannot poſſibly doubt.

"The lime, I conceive, is purpoſely introduced into the aſhes by the American manufacturers, not with the intention of a fraudulent adulteration, but with ſome view to the improvement of the article. It is, perhaps, added to the ley, with the view of clearing and ſtrengthening it; and part of it, being chemically diſſolved, will adhere to the clear ley, and even to the dry alkali. Now, a very few grains of carbonate of lime, in a hundred weight of pearl-aſhes, will totally unfit them for the preparation of magneſia; and a ſmall quantity of mortar, from the brick-work of the ovens, will ſpoil a very conſiderable quantity of alkali. We are peculiarly anxious to obtain a ſupply of pearl-aſhes, in the manufacture of which every ſource of lime has been carefully avoided. We ſhould even wiſh a careful trial to be made of each barrel, or, at leaſt, of each lot of barrels of one boiling, before the aſhes were ſent to us. This trial might be conducted as follows:—Diſſolve four ounces of pearl-aſh (we uſe none but the firſt ſort) in four ounces of diſtilled water, and four ounces of ſulphate of magneſia in the ſame quantity of water, mix the two ſolutions, and waſh off the ſulphate of pot-aſh by repeated affuſions of hot-water, which ſhould be quite free from carbonate of lime or ſulphate of lime: dry the magneſia, and calcine it during two hours in a ſtrong fire. If the calcined magneſia, when mixed with a little water, be abſolutely taſteleſs, the aſhes will anſwer. To aſcertain their purity deciſively, digeſt about a dram of the calcined magneſia in half an ounce meaſure, or more, of diſtilled water, for half an hour, ſhaking it occaſionally; filter the liquor, and blow air through it from the lungs, by means of a tobacco-pipe; and to another portion add a little oxalic acid. If neither of theſe teſts indicates lime, the magneſia is pure, and the aſhes may be purchaſed.

"To bring within a ſhort view what I have to requeſt, I ſubjoin the following queries:

"1. By what means does the carbonate of lime gain admittance into the American pot and pearl-aſhes?

" 2 If the ſource of it be known, is it one that can be eaſily avoided?

" 3. If the lime be purpoſely added, and this addition be, in general, neceſſary, could we not have aſhes manufactured expreſsly for our uſe, without this admixture?

" 4. Is the exportation of the unlixiviated wood-aſhes permitted; and could we not obtain a purer alkali by importing theſe wood-aſhes, and lixiviating them ourſelves?

" 5. Would you, or any other perſon you might point out, undertake to make, carefully, the neceſſary experiments, on the pearl-aſhes intended for us, before they are ſent from America? For ſuch aſſiſtance we ſhould be ready to make a liberal compenſation.

" 6. Suppoſing that it is poſſible to have an alkali made purpoſely for our own uſe, how long a time would elapſe between the order from New-York to have them manufactured, and their being ſent from an American port?"

DISCOVERIES IN MINERALOGY.

MOLYBDÆNA has been repeatedly found in the granitical rocks, which abound in the ſouthern parts of the State of New-York.—Beautiful ZEOLYTES have been lately diſcovered at Hobocken, filling up veins in the rocks of Telgſtein and Jade, which abound thereabout—and by the blowing down of a tree in the town of New-Marlborough, weſt of Poughkeepſie, and near the bank of the Hudſon, a *parcel of coal has been laid bare.* Smiths have worked this in their forges, and find it to be of a good quality. The ſpecimen of it ſent us, burned very well, affording much blaze, and emitting a bright light. Future reſearches and experiments can only determine whether this valuable material exiſts there in quantity ſufficient to ſupply the metropolis, and leſſen the extravagant and increaſing price of fire-wood.

EXTINCT SPECIES OF ANIMALS.

G. Cuvier, member of the National Inſtitute, and Profeſſor in the College of France, is engaged in a very extenſive work upon the ſpecies of quadrupeds whoſe bones have been found in the interior parts of the earth. Our readers will recollect that the State of New-York is now the theatre of the lateſt diſcoveries that have been made of foſſil bones; the particular accounts of which, as contained in the letters addreſſed to Dr. Mitchill, are inſerted in our preſent volume, p. 211. Mr. Cuvier has undertaken to ſettle the controverſy

concerning theſe animal relics. He ſays that the ſtrata of every country upon earth contain bones different from thoſe of the animals which now inhabit their ſurface: that, with the ſingle exception of ruminant animals, all the complete foſſil bones which he has ſeen are different from thoſe of quadrupeds now alive: that of theſe he has been able to aſcertain *twenty-three ſpecies*, all certainly unknown at this day, and which appear to have been entirely deſtroyed, though their bones evince their exiſtence in former ages.

Theſe ſpecies of creatures, whoſe races are now extinct, M. Cuvier divides into two claſſes—1. Thoſe which have been determined by others; and, 2. Such as have been ſettled by himſelf. In the firſt he enumerates the following: 1. The *Siberian animal* which affords foſſil ivory. 2. The *mammoth*, differing from the former chiefly in the ſize and points of its grinders. 3. The *long-headed rhinoceros*. 4. That animal of the *tardigrade family* called *megatherium* and *megalonyx*. 5. An extinct ſpecies of *large bear*. 6. *Another ſpecies* of the bear. 7. A *carnivorous animal*, intermediate between the wolf and hyæna. 8. A *creature a-kin to the mooſe*, whoſe horns meaſure fourteen feet from tip to tip. 9. The great *foſſil tortoiſe*. 10. The *maeſtricht crocodile*. 10. A *ſort of dragon*. 12. An unknown kind of reptile or cetaceous animal.—In the ſecond claſs, the chief of which have been diſcovered in France, Mr. Cuvier places the following ſpecies: 1. The animal whoſe *teeth, when impregnated with copper*, form the occidental turquoiſe. 2. A *tapir*, differing from that of South-America only in the form of its grinders. 3. *Another tapir*, of a gigantic or elephantine ſize. 4. A *ſpecies of hippopotamos*, of about the ſize of a hog. 5, 6, 7, 8, 9, 10. Six foſſil ſkeletons of an unknown ſpecies between the rhinoceros and the tapir, from the plaſter-quarries in the neighbourhood of Paris. 11. A ſpecies of crocodile, conſiderably like that of the Ganges.

But theſe are not all which the earth contains: there are *parts* of ſkeletons of which M. Cuvier cannot ſpeak with equal aſſurance; but of which, however, enough is known to encourage a hope that the liſt of zoological antiquities will be ſoon lengthened. Of theſe, ſome reſemble the bones, 1. Of the tiger. 2. Of an hyæna or ſea-calf. 3. Of the fallow-deer. And others of uncertain characters; as the petrified bones, 1. Near Verona. 2 and 3. Two ſorts in the Rock of Gibraltar. 4. In the vicinity of Dax. 5. Near Orleans. 6. Near Aix and Cette. 7. In the iſlands of Dal-

matia, &c. And, 8. All other *uncertain bones* found in the peat-moſſes of all parts of Europe and Aſia. In the courſe of a ſhort time M. Cuvier hopes to determine the exact place in the ſyſtem to which theſe doubtful ſpecies are to be referred.

M. Cuvier ſolicits information on theſe ſubjects from all parts of the world. He wiſhes to procure the bones themſelves, or figures of them, or correct deſcriptions in words.

THE AMERICAN BISON.

An animal of this ſpecies has been for ſome time exhibited in New-York. He is a male, and was taken on the weſt ſide of the Miſſiſſippi. He has a ſhaggy covering of hair on his pate, under his jaw, along his neck, and on the back part of his fore legs. His ſhoulders are very much elevated, ſo that the depth from the withers through to the briſket is greater, in proportion, than, perhaps, in any other creature. The hinder parts taper away, and are comparatively of a diminutive ſize. This biſon has covered a number of our domeſticated cows. He is ſtrong, and ſo capable of defending himſelf, that though he has often been baited by two bull-dogs at once, they have not been able to faſten him. He was taken when a calf, and is now ſo docile as to be eaſily managed by means of a ring in his noſe. He feeds upon graſs, hay, &c. like common neat cattle.

Some of the naturaliſts who have ſeen this animal have doubted to which of the *ſpecies* of the BOS family he belongs. It ſeems to be ſettled among the zoologiſts, that the genus of BOS is divided into *two important* ſpecies—the biſon and the buffaloe. To the biſons belong the varieties known by the names of *bonaſus* by Ariſtotle and the Greeks, of *urus* by Cæſar, of *aurochs* by the harſh modern imitators of that Latin name, of *biſon* by Pliny, of *bos ferus* by Strabo, and of *zebu*, or ſmall Barbary cow, by travellers from Africa. Theſe are affirmed to intermingle and breed with each other, and their offſpring are not mules, but can perpetuate their race. To the *buffaloes*, who ſeem to have been unknown to the ancients, belongs that ſpecies of ox who, about the ſeventh century, was imported from Africa into Italy, and domeſticated there, and known latterly by the very improper appellation of *bubalus*. Theſe have no ſexual or other intercourſe with the biſons; and are deſtitute of the elevated ſtructure of the ſhoulders.—Our neat cattle are the deſcendants of the biſons—not of the buffaloes.

The figure of the biſon, in Goldſmith's ſecond volume of his Hiſtory of the Earth and animated Nature, has a nearer reſemblance of the variety we are deſcribing than any we have ſeen. The figures of the urus and biſon, in Bewick's Hiſtory of Quadrupeds, are of the ſame general character; and the latter, particularly, has a near likeneſs to this native American. The withers of the former are not high enough. In two editions of Buffon and Daubenton's Zoology (vol. xxiii.), which we have conſulted, there is a figure of the buffaloe, but not of the biſon. Pennant, in his Arctic Zoology, vol. iii. has deſcribed the biſon very well; though it is preſumable there are more varieties of the breed than we are yet acquainted with. It is a remarkable fact, that with ſuch a creature in their neighbourhood, of a temper ſo very docile, and capable of becoming ſo ſingularly uſeful to man, the aborigines of the weſtern continent ſeem never to have thought of taming it! The domeſticated kine of America are all deſcended from the milch cows of Europe. The biſons ſtill continue in their ſavage ſtate, and conſtitute one part of the cattle of the foreſts; while the offſpring of imported cows, turned out to range at large in the wilds of Brazil and La Plata, and gradually returning to the ſtate of freedom and fierceneſs, forms the other.

METHODS OF DECOMPOSING MURIATE OF SODA (COMMON SALT.)

M. Carendeffez has made many experiments on this ſubject, with the intent to procure ſeparately both the acid and the alkaline baſis of ſea-ſalt. He thinks he has made ſome obſervations which are peculiar to himſelf.

Subſtances capable of decompoſing this neutral ſalt are divided by him into claſſes. In the *firſt* are five ſubſtances—*pot-aſh*, *barytes*, *oxyd of lead*, *lime* and *iron*. In the *ſecond* claſs he places ſuch things as can effect the decompoſition, though in an indirect manner: theſe are ſix—the *boracic*, *phoſphoric*, *arſeniac*, *nitric*, *ſulphuric* and *tartaric* acids. The latter of theſe decompoſes not only the muriates of potaſh and ſoda, but likewiſe the ſulphates and nitrates of theſe alkalies, ſo as to form acidulous new compounds with them reſpectively, convertible into oxalic acid by the aid of the nitric. In the *third* claſs he ranks the *ſulphates of pot-aſh*, *of alumine*, *of copper*, *of iron*, *of magneſia*, and *of ammoniac*; and the *ſulphures of the ſame metals*.

He alſo finds that the nitrates of ſilver and of quickſilver

have the like property, as well as clay and all the earthy ſulphates. Theſe experiments ſhow how far chemiſtry has penetrated in this inquiry; and he who undertakes the decompoſition of ſea-ſalt, has it now in his power to chooſe from theſe modes of operating the one which to him ſhall appear preferable. Though ſoda is found *native* in Egypt, Barbary, Turkey, Perſia, India, Siberia, Hungary, and ſome other places, it has long been highly deſirable to obtain it *by art*. The river Belus, in Judea, whoſe ſand is remarkable for the manufacture of glaſs, is mentioned by the hiſtorian Tacitus. This ſand is ſaid to be a natural mixture of the ſilicious girt with ſoda; and it is upon the ſhore of this ſtream that Pliny places the fabulous ſtory of the accidental diſcovery of the art of making glaſs by the Phenician merchants. We mentioned, in page 82, that in Egypt the muriate of ſoda ſeemed to be decompoſed by the carbonate of lime.

SHORT PROCESS FOR MAKING STARKEY'S SOAP.

This kind of ſoap conſiſts of the combination of an *eſſential oil* with a *fixed* alkali. The old proceſs for making it was difficult and tedious, requiring long trituration. M. Geoffroy diſcovered that the difficulty of combining the two materials aroſe from the employment of the *carbonates* of the alkalis inſtead of the naked or cauſtic ſalts. For if ten parts of pot-aſh, deprived of its carbonic acid, be rubbed in a hot mortar with eight parts of oil of turpentine, the ſoap may be formed at once. M. Carendeffez employs the ſame proportions; but he powders the cauſtic pot-aſh very fine, paſſes it through a ſilk ſeive, and forms inſtantly this compound, which is eſteemed, by many, an excellent external remedy for rheumatiſms, &c.

MORE PROOF OF THE UTILITY OF ADMINISTERING OXYGEN IN SCURVY.

It was mentioned in our preſent volume, p. 79, that a method had been diſcovered of procuring the citric acid in a concrete ſtate. The application of this diſcovery to the cure of ſcorbutic diſeaſes is confidently related by Dr. Trotter, Phyſician to the Britiſh fleet, in a letter to Mr. Coxwell, of London, the diſcoverer:

"*Plymouth-Dock, July* 13, 1800.

"SIR,

"With much pleaſure I now inform you of the complete ſucceſs of your concrete acid of lemons in the cure of the

ſcurvy, which, in every reſpect, has equalled, if not ſurpaſſed, the uſual effect of the freſh lime, lemon, or Seville orange-juice.

"It is no ſmall ſatisfaction to myſelf, from the nature of my ſtudies for the laſt ten years, to find that my ſervice has been protracted to ſee the final blow to this ſea diſeaſe. In juſtice to his majeſty's navy at large, I have therefore addreſſed the board of admiralty on the ſubject, and I have announced the ſucceſs of my experiments to the medical world, through the London Medical and Phyſical Journal. You are extremely welcome to make what uſe you think proper of this letter.

"I am, with much reſpect,
"Sir, your very humble ſervant,
"T. TROTTER, M. D."

It ſeems, from this teſtimony, that the lemon-juice loſes none of its virtue during the preparation. To be enabled to carry oxygen, thus concentered and impriſoned, into the holds of ſhips at ſea, for the invigoration of ſickly mariners, is a great improvement in nautical medicine, and will have a tendency to leſſen a large proportion of the diſtempers to which ſailors are victims.

It is in this, and the kindred forms of febrile diſeaſe, that we believe the acid remedies recommended by Profeſſor Reich (p. 314 & ſeq.) would be particularly ſerviceable. And the *citric* promiſes much more ſucceſs than the *ſulphuric* and *muriatic* acids. Reich's general reaſoning on the ſubject has great appearance of truth in it, as far as *one certain form* of malignant diſtemper extends; but he would be more likely to win the aſſent of his readers, if he limited his doctrine to *the ſcorbutic form of fever*, as Trotter does, inſtead of extending it to fever univerſally. That acids are highly ſerviceable in many kinds of febrile malady, there is as full evidence as of any fact in phyſic. What theſe ſtates of body are, both Reich and Trotter have, in their reſpective publications, attempted to explain. In Dr. S. Brown's (of Boſton) Treatiſe on the Yellow Fever, there is a letter of Dr. Mitchill's, not publiſhed any where elſe, containing ſome ideas of that diſtemper as prevalent at New-York in 1798; *one* of the forms of which is thus deſcribed:

"'Symptoms of torpor, liſtleſſneſs, coma, low delirium, moderate heat, *little*, and, in ſome inſtances, *no* pain; extreme muſcular debility, want of irritability, deſtruction of the vis inſita, dulneſs of ſenſation and perception, yellowneſs of the

eyes and ſkin in moſt caſes, without pain in the right hypochondrium, ſhortneſs of breath, with little or no ſpecial diſorder, either in the blood-veſſels or alimentary canal. This train of ſymptoms forms one of the moſt inſidious modes of the diſtemper, the patient declaring, that little or nothing is the matter with him, though verging faſt upon diſſolution. In ſuch a dangerous ſtate of the conſtitution, I have, from much obſervation and much reflection, been led to believe, that the *nerves* and *muſcles*, as well as the *blood*, are *over*charged with ſepton, or *under*charged with oxygen; whence a rationale of the torpor, &c. can be eaſily deduced. The indication of cure, then, will be the ſame as in ſcurvy; that is, to introduce oxygen and to ſubduct ſepton. For, I believe, and you will, I think, find it true, that the form of diſeaſe now under conſideration might, with propriety, be denominated *acute ſcurvy*, in contradiſtinction to the *ordinary* or *chronic ſcurvy*, and receive a ſuitable treatment. I have preſcribed *neutral mixtures*, *lemonade*, *cyder*, *peaches*, *pears*, and *apples*, for ſeveral patients, moſtly to try whether this idea is founded in fact."—*Brown's Treatiſe, &c. p.* 10.

NITRIC ACID NOT TO BE RELIED ON TO CURE LUES VENEREA.

Dr. Rodgers, Profeſſor of Obſtetrics in Columbia College, and Lecturer on Clinical Medicine in the New-York Hoſpital, has made many trials with nitric acid in ſyphilis. The general reſults of his experience, both in the hoſpital and in his private practice, are, that this acid does not appear to exert its good effects where the conſtitution is in much tone, or where the inflammatory diatheſis prevails; that its benefits are moſt evident where the habit of the body generally, or any particular part thereof, has been much debilitated by the long continuance of diſeaſe, or by the operation of remedies; and that it is not to be relied on as an antiſyphilitic remedy, eſpecially in recent caſes, or where the patient is of a ſanguine temperament, though it is often of the greateſt ſervice in all debilitated and diſoxygenated habits, where, *after the uſe of quickſilver*, the body has acquired great irritability, and may be ſaid to be MERCURIALIZED. The communication of Profeſſor Rodgers, on this ſubject, will be inſerted in the firſt number of our fifth volume.

PLAGUE NOT CONTAGIOUS.

Dr. D. Whyte, in a communication made near the end of the year 1800, from the Pera of Conſtantinople, declares his conviction decidedly, that the plague is in no inſtance either an incurable or contagious diſeaſe. Some of the perſons, interrogated by Mr. Howard, ſeemed to entertain a ſimilar opinion. Latterly, theſe doubts are more boldly expreſſed even in Europe. In all the accounts of the plague we have ſeen, even in the quartos of the Ruſſels, there is very little to be found of thoſe ſubjects, concerning which, a ſenſible inquirer wants information; we mean of the food, way of life, dreſs, habitations, materials of building, manner of waſhing clothes and cleaning houſes, and of the phyſical geography of the places where the plague prevails. Until theſe are known, we can never know much, correctly, of the plague of Aſia. The accounts given of it by the Franks are, beyond a doubt, very partial, or imperfect in ſome reſpects and exaggerated in others; and, for the moſt part, tinctured with prejudice or enmity againſt the Turks. There is ſtrong reaſon to believe, that great miſchief is engendered *on board* the filthy veſſels engaged in the Mediterranean trade; and it is well known, that ſome of the cells and apartments of Lazarettos, are among the foulеſt of human abodes. Ships rendered foul by innate poiſon; this poiſon, concentrated and worked up to great malignity by confinement during a quarantine, and the paſſengers and crew immured in an execrable peſt-houſe, are enough, of themſelves, to produce a plague in any region almoſt of the globe. There is ſtrong ground to believe, that the moſt conſiderable ſhare of the plague complained of among the Chriſtians, is both produced and perpetuated under *their own ill-judged regulations*, and then moſt prepoſterouſly ſaid to be imported from Syria, Egypt, or Greece, where, among the Mohammedans it is pretended, its contagion is never extinguiſhed; but it is high time to get the better of theſe deluſions. A little more ſcience, and a few good obſervers will decide this queſtion, and reſcue philoſophy, commerce and navigation, from the terrors of imported contagion.

NOXIOUS QUALITY OF THE SALIVARY DISCHARGE.

Communicated to the Editors of the Medical Repoſitory by Mr. Abel Catlin, of Lichfield (Connecticut.)

"The deleterious effects of the *ſeptic acid* are," writes Mr. Catlin, "I think, ſatisfactorily aſcertained, and ſcarcely

need a proof be offered to confirm the doctrine of the exiſtence of that principle, which is capable of deſtroying animal nature; and was it not for the curious and well-defined manner in which it operated in the following inſtance, I would wave the communication, as unneceſſary to eſtabliſh a doctrine that bids fair to be long retained by the philoſophical world.

"During the laſt Auguſt and September I ſuffered a long and violent fever, attended with very malignant and nervous ſymptoms; and though then incapable of any obſervation myſelf, I had the ſymptoms and circumſtances of the diſeaſe very accurately related to me by the gentlemen who attended me. The detail of theſe I conſider it unneceſſary to mention for the purpoſe of the particular object of this communication. That object I expreſs thus:—Sometime about the height of my diſorder, the expectoration and ſalival diſcharge, which were very great, *did, for one day, and two hours of the next, inſtantly kill every fly that in any manner came in contact with them*, (which, from the ſeaſon and other circumſtances, were very numerous). A ſweat that then broke out terminated its noxious effect on the flies.

"It will be needleſs to mention the excoriating effect of a diſcharge thus virulent on my mouth and throat, and to expreſs my wonder and gratitude, that a ſyſtem, fraught with ſo deadly a principle, ſhould ſurvive."*

For the four following articles we are obliged to Dr. John Vaughan, of Wilmington, in the State of Delaware.

PARTURITION.

In 1797 I was called to a woman in labour; and, ſhortly after my arrival, ſhe had a daughter: and while I was ſeparating this child, and handing it to the midwife, two boys were born with a ſingle pain. They lay parallel to each other, and the navel ſtrings were wrapped about their necks, and knotted as completely as if it had been done by art.

They appeared to have been ſtrangled at about the ſeventh month of geſtation.

There was but one placenta, into which the umbilical chords of the boys entered in contact, while that of the girl was three or four inches diſtant.

The laſt year has been remarkable, in this neighbourhood, for tedious and difficult labours. The difficulty was generally

* Was not quickſilver adminiſtered in this caſe? EDIT.

owing to a rigidity of the *os uteri*; and in ſeveral caſes, to which I was called as a coadjutor, the midwives had neglected the evil until the orifice of the womb was puſhed back towards the *ſacrum*, and the child's head protruding, with the womb before it. But by reſiſting the deſcent of the child during the pains, and very gradual preſſure upwards in their abſence, until the *os uteri* was brought over the vertex, and afterwards preſerving it in a proper ſituation, the womb dilated, and all difficulty was overcome.

In ſeveral caſes of rigidity of the *os uteri*, without inverſion, blood-letting anſwered a valuable purpoſe in producing relaxation. In one deplorable caſe of this kind, to which I was called in the evening, after drawing eight ounces of blood from the arm, which produced faintneſs, I endeavoured to dilate very gradually; but the pains were intolerable, and in about an hour the unfortunate woman was attacked with ſevere convulſions. My ſituation was now perplexing in the extreme. The attendants gave her up for dead, and left the room; and, being without aſſiſtants, I tied up her arm, and let it bleed in the bed until the ſpaſms ceaſed, and immediately delivered her of a very large child. It was ſaid to weigh 28lbs.—This woman had a ſpeedy recovery.

The only fatal caſe of parturition which I have had the misfortune to witneſs was ſo peculiar that it deſerves communication, and may be an uſeful leſſon to others as well as myſelf.

Mrs. ——, the mother of two or three children, was delivered of twins at eight o'clock in the evening; and a tumour remaining in the uterus, the midwives (there were two) ſuppoſed there was a third child, and ſent for me at two next morning. On examining the uterus externally, it was of an irregular form, and of a ſufficient ſize to excite ſuſpicion of a third child. But the woman not being ſenſible of any motion in the uterus, and free from pain—and having met with ſome previous caſes where the effuſion of blood, from a partial ſeparation of the placenta, into the uterus produced a conſiderable tumour, I decided in the negative, and wiſhed to bring away the placenta as ſoon as convenient. However, the midwives and relations controuled, and propoſed ſending for another phyſician; and he agreeing with the midwives, I gave up to the deluſion; and while we were waiting for a return of labour, the poor woman expired without a ſtruggle or a groan. On examination after death, I found the placenta partially ſeparated, and the uterus filled with coagulated blood.

There is one circumſtance to which I requeſt the attention of my obſtetric brethren, *the effects of vomiting during labour*. It is proverbial with the old women, that vomiting is a favourable ſymptom, and I have frequently ſeen a tedious labour terminated by ſpontaneous vomiting:—hence, the following query has ariſen. Would not emetics, or nauſeating doſes of ipecacuanha, be ſerviceable in removing rigidity of the os uteri, and in dilating the vagina and *os externum*? The effects of nauſea, in moderating febrile action, are univerſally admitted, and we are authorized to infer, that it would be highly ſerviceable in many caſes of parturition. The peculiar ſufferings of the ſex demand our ſpecial attention, and we ſhould be aſſiduous in our endeavours to leſſen their violent pains.

AURA EPILEPTICA.

Miſs M——, at the age of puberty, was attacked with ſpaſms in the fore finger of the right hand, which gradually extended up the arm to the right ſide of the head, affecting the muſcles of the eye lids particularly. During one of thoſe paroxyſms, I placed her on the inſulating ſtool, in connection with the prime conductor of an electric machine, and drew ſparks from the affected hands. In a few minutes the ſpaſms ceaſed, and returned but once afterwards.

GUTTA SERENA.

In February laſt I was applied to by a young lady, who had loſt her ſight without any manifeſt cauſe or apparent injury to the eyes. Electricity preſented itſelf as the moſt probable remedy; and I at firſt drew ſparks from the eye-lids with a wooden point, and afterwards with a metallic one. Its effects were immediately beneficial, and a few electrifications cured her.

LUSUS NATURÆ.

In the fall 1797, I was ſent for to a negro child that had an extra finger on each hand, growing at right lines from the little fingers. They were about $\frac{3}{4}$ths of an inch in length, contained an artery and vein, had perfect nails, but were deſtitute of bone. They were taken off by ligature.

A few weeks ſince I diſſected a child aged thirteen months, that had been diſtreſſed with difficult reſpiration from its birth. On diſſecting off the integuments, to expoſe the cavity of the thorax, I was ſurprized to find it deſtitute of a ſternum, and

the ribs united by a ſmall cartilage. The lungs were flattened—the pericardium contained an unuſual quantity of liquor, and the heart was preternaturally large. The abdominal viſcera were, alſo, much diſeaſed—the liver enlarged, and riſing upwards againſt the mediaſtinum, leſſening the thoracic cavity; but the ſpleen was ſmall and dark coloured.

NEW PUBLICATIONS INTENDED.

The Medical Faculty of the Univerſity of Pennſylvania have it in contemplation to make a collection of the beſt Inaugural Diſſertations of their graduates, and to republiſh them in volumes. The propriety and uſefulneſs of this was hinted in the review of one of their collegiate exerciſes, in Med. Rep. vol. ii. p. 197, firſt edit. The ingenious and excellent matter which many of them contain, render them valuable articles for ſuch a Theſaurus.

Profeſſor Woodhouſe intends to publiſh an American Diſpenſatory. It will conſiſt of three parts. 1. An elementary treatiſe on Chemiſtry, and its application to Pharmacy. 2. A hiſtory of ſubſtances employed in Materia Medica, with an account of a number of the active indigenous plants of the United States, and a theory of their action on the human body. 3. An account of the beſt medicinal preparations employed in private practice, in hoſpitals, and of various foreign pharmacopœias, with a plate of chemical apparatus. Dr. Woodhouſe expects to put it to preſs ſome time in the enſuing autumn, or next ſpring.

Dr. Barton, Profeſſor of Materia Medica, Natural Hiſtory, and Botany, in the Univerſity of Pennſylvania, will ſpeedily put to preſs, a work, entitled, "*Elements of Botany.*" This work, the plan of which is eſſentially different from that of the elementary works on Botany now in uſe, will be elegantly printed, in one volume large (royal) octavo, of at leaſt 280 pages, and will be illuſtrated by a number (not leſs than eighteen) of plates, engraven by an able artiſt.

NEW COURSE OF LECTURES.

Dr. Meaſe, of Philadelphia, intends to deliver, in the enſuing autumn, a courſe of Lectures, "*on the Application of the Principles of Natural Philoſophy and Chemiſtry, to the common Purpoſes of Life.*" Theſe Lectures will be nearly on the plan of thoſe delivered by Dr. Garnett in the "Royal Inſtitution" of Great-Britain.

FOREIGN.

NITROUS ACID AND OPIUM IN DYSENTERY.

IN the *Medical and Phyſical Journal* for May, 1800, there is a remarkable paper on the treatment of dyſentery, by a combination of nitrous acid and opium. The author (Dr. Hope, of Chatham) intimates a ſuſpicion, that the dyſentery and yellow fever are produced by the ſame cauſes, operating under different circumſtances. In all the caſes, reported in this paper, where the efficacy of this new mode of treatment was obſerved, the diſeaſe was ſomewhat advanced, and the more common remedies had been all ineffectually preſcribed. The firſt occurrence of this acid to the author's mind was ſuggeſted by the ſymptoms of extreme thirſt in a caſe of dyſentery, and the addition of opium was merely intended as a corrective of the acid. The ſignal advantages, however, of the compoſition, ſoon induced him to extend the uſe of it. The formula was as follows:—℞ Acid. Nitr. ʒ ij. Opii gr. ii. Aq. puræ, ℥ ij. M. ft. cap. coch. 1. m. ter quaterve die in vehiculo.

REPUBLICAN LYCEUM AT PARIS.

During the year 1800, which was the ſixteenth from its eſtabliſhment, the branches of knowledge taught, and the names of the Profeſſors in this inſtitution, were the following:

1. Experimental Phyſics, or Natural Philoſophy,	Profeſſor Citizen Butet.
2. Chemiſtry,	Fourcroy.
3. Natural Hiſtory,	Cuvier.
4. Anatomy and Phyſiology,	Sue.
5. Hygiene, or the art of preſerving health,	Moreau.
6. Technology, or the application of ſcience to arts and trades,	Haſſenfratz.
7. Education, and the phyſical and moral faculties of man,	Le Roy.
8. Literature,	La Harpe.
9. Hiſtory,	Garat.
10. Public or Political Economy,	Roederer.
11. Moral Philoſophy,	Degerando.

12. The art of Deſigning, particularly in Architecture, both ancient and modern,	Legrand.
13. Engliſh Language,	Roberts.
14. Italian Language,	Boldoni.

END OF VOLUME IV.